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ERRATA

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VOL. XXVII.   PART I.

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On pag 94, line 35, for *iron* read *ion*.

,, ,, 97 ,, 24 ,, *proportion* read *proportional*.

,, ,, 100 ,, 20 ,, *induction* read *inductive*. ·

,, ,, 101 ,, 34 ,, *unlike* read *unlikely*.

,, ,, 111 ,, 25, insert *pulse* after *Rontgen*.

The quantity $\sqrt{M\Omega}$ is in the denominator of all the expressions in which it occurs on page 95 *et seq.*

XIII. *On the Criterion of Steel Suitable for Permanent Magnets.*
    By Silvanus P. Thompson, *D.Sc., F.R.S.*

In order that a steel shall be suitable for the making of permanent magnets, the prime requisites are that it shall have a large remanent magnetization and a high coercive force. These two qualities are independent of any question as to form. But it is well known that bars which are short have but a small remanent magnetization owing to their self-demagnetizing coefficient being great. In comparing together steels of differing composition, or steels of identical composition which have been subjected to different heat-treatments, it is therefore necessary to assume that the specimens to be compared are of similar form. According to all evidence to the present time, the coercive force of any given specimen of steel is independent of the form, provided that it has been subjected to a sufficiently great magnetizing force, the self-demagnetizing coefficient affecting the degree of remanent magnetization, but not affecting the degree of coercion with which that residuum is held by the steel.

When any specimen of steel is subjected to a cycle of magnetization so as to obtain its hysteresis loop, the dimensions of the loop—provided the maximum magnetizing force was sufficiently great to enable a practical saturation to be attained during the magnetizing process—suffice to form a judgment as to its suitability for the purpose of making permanent magnets. A field of intensity $\mathcal{H}=500$ is amply sufficient for any kind of steel; and for any but the very hardest a field of $\mathcal{H}=250$ is adequate. Fig. 1 will make the matters at issue plain. It represents, for a fine quality of magnet steel, the descending branch of a hysteresis loop, the ordinates being values of $\mathcal{J}$, the "magnetization" (magnetic moment per cubic centimetre) and the abscissæ the values of $\mathcal{H}$. The value of $\mathcal{J}_{rem.}$ the remanent magnetism (for a ring or a very long bar), is the ordinate OR, and the coercive force $\mathcal{H}_c$ is the abscissa OC. In this steel $\mathcal{J}_{rem.}=808$, and $\mathcal{H}_c$ is 63. The lines which slope upward to the left from the origin represent graphically, by the respective tangents of the angles of their slope, the values (to a scale of $\frac{1}{10}$) of the coefficients of self-demagnetization for bars

of about 10, 12, 15, 25 and 35 diameters' length respectively.
The abscissa at the point P, where the sloping line crosses the
curve, shows the residual magnetization for a bar that is 25
diameters long which has a coefficient of self-demagnetization
of about 0·05. At this point the residual magnetization is
reduced to about 640, and the demagnetizing force will be
about $640 \times 0·05 = 32$.

For all specimens of steel the hysteresis curves will be of the
same generic form ; but the two chief parameters OR and OC,
that is $\mathfrak{J}_{rem.}$ and $\mathfrak{K}_c$, will differ. In the accompanying table*
are given the values of $\mathfrak{J}_{rem.}$ and $\mathfrak{K}_c$, which have been found by
various observers—Ewing, Du Bois and Taylor Jones, Mme.
Curie and others—for a number of different brands of iron and
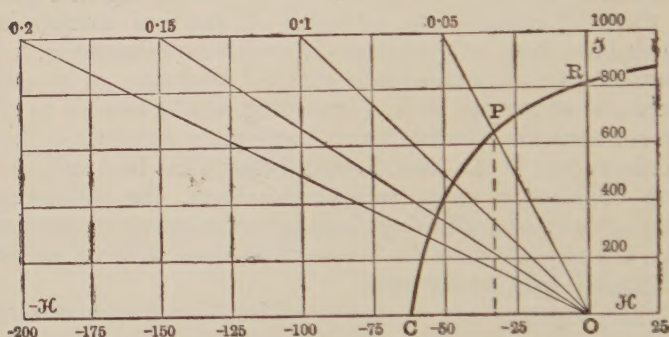
Fig. 1.

steel. The highest value found for $\mathfrak{J}_{rem.}$ is 900, in a specimen of
unquenched Allevard steel (carbon content 0·59 per cent.,
tungsten content 5·5 per cent.) ; but it had only 26 for $\mathfrak{K}_c$.
The highest value of $\mathfrak{K}_c$ is 85, for a molybdenum steel (carbon
1·25, molybdenum 3·36 per cent.), but its $\mathfrak{J}_{rem.}$ was only 370.
No steel has yet been found which comes up to the quite
possible ideal of having $\mathfrak{J}_{rem.} = 800$, with a coercive force of
$\mathfrak{K}_c = 80$. The best magnet steel will have the values of both
parameters high. For short magnets where the self-demag-
netizing influences are great, it is more important that $\mathfrak{K}_c$
should be high, than that $\mathfrak{J}_{rem.}$ should be great.

* Originally given in the Author's Paper on the "Magnetism of Per-
manent Magnets," Journal of Institution of Electrical Engineers, Vol. L.,
p. 80, 1913.

Material.	$H_c.$	$I_r.$	$H_c \div I_r.$	$H_c \times I_r.$
Swedish wrought iron	0·8	550	0·00145	440
Softest selected iron	0·44	804	0·00547	322
Piano steel wire	22·0	824	0·02670	1,813
Piano steel wire	40·0	760	0·05260	30,400
Low-carbon steel	3·4	625	0·00544	1,900
High-carbon steel	58·0	645	0·09000	37,400
Haarlem magnet steel	56·0	800	0·07000	44,800
Allevard steel	26·0	900	0·02890	23,410
Allevard steel	73·0	800	0·09120	58,400
Böhler's Styrian steel	34·0	790	0·04300	26,860
Böhler's Styrian steel	75·0	600	0·12500	45,000
Remy's tungsten steel	63·0	808	0·07790	51,000
Remy's tungsten steel	77·0	800	0·09620	61,600
Medium tungsten steel	58·8	572	0·10300	33,600
Whitworth tungsten steel	37·0	610	0·06070	22,550
Molybdenum tungsten steel	85·0	370	0·23000	31,400
Chilled cast iron	52·5	218	0·24300	11,530
Lodestone	50·0	350	0·14300	17,500
High carbon steel, 1·2 per cent. C., quenched at 905°C.	48·0	264	0·18100	12,650
Cast iron (Hopkinson)	3·8	312	0·01230	1,190
Manganese steel annealed (Hopk.).	24·5	43	0·57200	1,054
Grey cast iron (Hopkinson)	13·67	250	0·05460	3,410
Chrome steel (Mathews, 7th)	52·5	690	0·07620	36,250
Tungsten steel (Mathews, 1st)	71·5	806	0·08870	57,700
Chrome steel tempered (Mathews, V., 3rd)	22·0	1,030	0·02135	22,660
Chrome steel (Miss Moir)	56·0	286	0·1954	16,016

Recently the question of the suitability of steels for permanent magnets has been discussed by Mr. J. A. Mathews, in the Proceedings of the American Society for Testing Materials, Vol. XIV., p. 51, 1914, who suggests that a new criterion of magnetic hardness should be framed upon the basis of the ratio between $\mathfrak{B}_{rem.}$ and $\mathfrak{H}_c$, or on the inverse ratio of $\mathfrak{H}_c \div \mathfrak{B}_{rem.}$, preferably the latter, as the magnetically harder steels then have the higher number. He proposes to call this ratio the " permanence."

Mr. J. R. Ashworth has made precisely the same suggestion in " Nature," January 7, 1915, p. 506, with the difference that he states the " permanence " in terms of the ratio $\mathfrak{H}_c \div \mathfrak{J}_{rem.}$. But, as $\mathfrak{B}_{rem.} = 4\pi \mathfrak{J}_{rem.}$, the distinction is one of scale only. Mr. Ashworth explicitly suggests this ratio as the criterion for " determining the most suitable material from which to construct a permanent magnet."

Now, ever since Hopkinson's determinations 30 years ago, it has been supposed that for purpose of making permanent magnets the best material would be that for which both $\mathfrak{J}_{rem.}$

and $\mathcal{H}_c$ should be high ; and for which the product $\mathcal{H}_c \times J_{rem}$ was, therefore, a maximum.

To decide between this view and the new suggestion to take the ratio as criterion, instead of the product, the values of $\mathcal{H}_c \div J_{rem}$ and of $\mathcal{H}_c \times J_{rem}$ have been added to the table. The results are illuminating. Two of the materials—viz., molybdenum steel and chilled cast iron, are seen to have the ratio $\mathcal{H}_c \div J_{rem}$ nearly equal—viz., 0·23 and 0·243 respectively. But their products $\mathcal{H}_c \times J_{rem}$ are 31,400 and 11,530 respectively ! And as molybdenum steel is 1·7 times higher than chilled cast-iron in the quantity of magnetism it retains, it follows that to produce two magnets of equal pull of these materials the cast-iron one must have $1·7 \times 1·7 = 2·87$ times the cross-section of the molybdenum steel ; and, even then, would have only 0·6 as great a coercive force ! Or, again, according to the proposed criterion of Mr. Mathews and Mr. Ashworth, annealed manganese steel, which has a ratio of 0·572, should be six times as good as Remy tungsten steel, which has a ratio of 0·096. But the manganese steel has a remanence of only 43, compared with 800 for the tungsten steel—that is, 18½ times as little— and to make the two magnets of equal power the magnet of manganese steel would have to be constructed with a sectional area 314 times as great as that of the tungsten steel ! The respective values of the product $\mathcal{H}_c \times J_{rem}$ for these two steels are 1,054 and 61,600 respectively ; or, as about 1 to 60. If we have to choose a criterion between manganese steel, which is almost non-magnetic, and the Remy steel, which is one of the finest magnet steels known, we should choose the one which makes out that the Remy steel is 60 times as good as manganese steel, rather than that which pronounces manganese steel to be six times as good as Remy steel.

Very little is known about the magnetism of such substances as basalt and terra-cotta ; but it is known that their remanent magnetization is very slight and their coercive force relatively enormous. But, because for them the ratio $\mathcal{H}_c \div J_{rem}$ probably exceeds that of any known brand of steel, no one would dream of recommending these as the most suitable materials for making permanent magnets.

It was suggested above that a possible ideal steel for permanent magnets is one which will have $J_{rem} = 800$ with $\mathcal{H}_c = 80$; here the ratio is 0·1 and the product is 64,000. I believe the highest J_{rem} yet recorded is Mr. Mathews's value of $J_{rem} = 1,030$.

This was for a chrome steel that had $\mathcal{H}_c$ only 22. The highest value of $\mathcal{H}_c$ yet observed is Mme. Curie's figure of 85 for a molybdenum steel; but its $\mathcal{J}_{rem.}$ was only 370. If it were possible for metallurgists to produce steel with $\mathcal{J}_{rem.}$ 1,000 and $\mathcal{H}_c = 100$, the value of the ratio would still be only 0·1 ; but the product would then be 100,000, and for equal power the sections and weights might be reduced to about 64 per cent. of those of our best permanent magnets of to-day. Or, for given section and weight, magnets made of this ideal steel would have about 24 per cent. greater pull, and 20 per cent. greater coercive force.

ABSTRACT.

Whatever the form to be given to a permanent magnet, the prime requisites as to the quality of the steel are (1) large remanent magnetism ($\mathcal{J}_{rem.}$) and a high coercive (force $\mathcal{H}_c$). Since Hopkinson's determinations of 1885 it has been supposed that for the purpose of making permanent magnets the best material would be that for which both of these quantities and, consequently, their numerical product, should be as high as possible. Recently Mr. J. A. Mathews and, independently, Mr. J. R. Ashworth, have proposed to take the ratio $\mathcal{H}_c \div \mathcal{J}_{rem.}$ or $\mathcal{H}_c \div \mathcal{B}_{rem.}$ which only differs in scale, as the criterion.

To decide as to the suggestion to take the ratio instead of the product a table giving the values of $\mathcal{H}_c$, $\mathcal{J}_{rem.}$, $\mathcal{H}_c \div \mathcal{J}_{rem.}$ and $\mathcal{H}_c \times \mathcal{J}_{rem.}$ for a number of steels is given, and it is clearly shown that the use of the ratio as a criterion of magnetic usefulness leads to most absurd results. As an example, annealed manganese steel (almost non-magnetic) should, judging from the ratio, be six times as good as Remy steel, whereas in reality, for equality of pull, a magnet of manganese steel would require to be 312 times the weight of that made of Remy steel. The qualities requisite in an ideal steel for permanent magnets are indicated.

DISCUSSION.

Dr. S. W. J. SMITH said that in cases (by far the most numerous) in which it was as essential that the moment of the magnet should be high as that it should be permanent, the criterion $H_c \div I_r$ a maximum obviously had very little to be said for it in comparison with the criterion $H_c \times I_r$ a maximum as Prof. Thompson had so clearly shown. It was possible, however, to think of cases where a " permanent magnet " might be required in which the constancy of the moment might be much more important than its magnitude. In these cases more might be said in favour of the first criterion. He pointed out that the choice between two different materials for a particular purpose might depend as much upon the shape of the IH curve as upon the values of I_r and H_c.

Mr. A. CAMPBELL said that for practical purposes it was important that I_r should be large or else a large coercive force was of no use. He agreed therefore that the product of H_c and I_r was the most useful criterion, and could not think of a case where the quotient would give a useful indication of the quality of the steel.

Dr. C. Chree said that peculiar cases were often encountered in which it was difficult to say exactly what interpretation should be put on the terms " permanent magnetism," " coercive force," &c. Some years ago he had had occasion to demagnetise a bar of cobalt. He had used the ordinary method of reversals, accelerating the process by hammering. By this process he actually reversed the magnetisation, a phenomenon which he attributed to the original magnetism being in the opposite direction in the outer layers to what it was in the inner core, the magnetism of the outer layers being destroyed by the hammering. He had also found with a certain dip needle that it could be magnetised more strongly in one direction than in the other. It was afterwards found that the steel from which the dip needle was made had been magnetised very strongly in that direction by the maker. There was another factor of importance besides the permanence of the moment of a magnet, and that was permanence of the distribution of the magnetisation. He cited the case of two Kew magnetometer magnets which had inadvertently been allowed to touch. Their moments were unaltered, but their mutual influence was considerably affected owing to a change in the distribution of the magnetisation.

Mr. E. H. Rayner communicated the following : From a practical point of view a high value for the product of remanence × coercivity is to be desired with the proviso that a low value in one of them can not compensate for a corresponding unusually high value in the other. A " figure of merit " might be made out by penalising the material in both of these properties, counting only after a certain value has been attained. This figure might be calculated from the formula $(I_r - 400) \times (H_c - 20)$. This would give a positive figure of merit to 15 materials in the list given in the Paper, implying that from a practical point of view the rest were comparatively worthless. The order of those with a positive figure of merit is much the same as the order of the magnitudes given in the last column of the Paper, the first four being in the same order and the fifth and sixth inverted. The most notable exception is the molybdenum tungsten steel which comes tenth in order of merit in the last column of the Paper, but which by reason of having a value for I_r of less than 400—namely, 370—has a negative figure of merit on the basis suggested. It would be interesting if Prof. Thompson could give some idea of what is the minimum value for H_c which is desirable in a good permanent magnet for commercial purposes such as magnetos for explosion engines.

Mr. J. R. Ashworth communicated the following : Prof. Thompson has discussed two criteria of steel suitable for permanent magnets, but has only applied one test in deciding upon their merits—namely, the pull which the magnet can exert. On the other hand, when I suggested the ratio of H_c to I_r as the definition of " permanence," I had in view, as I distinctly stated, " the preservation of the magnetism in a magnet," which for some purposes is more important than a high pulling force, for which I set up no criterion. From the standpoint of constancy, the ratio H_c to I_r may prove useful. For example, in soft iron the ratio is 0·0021 and in hardened iron 0·0115—5·5 times as great ; the products of H_c and I_r are 1,710 and 1,840 respectively. Here the products are not very different, and it is the ratio which shows that hardened iron is more suitable for a *stable* magnet than soft iron. Again, I have shown that chilled cast-iron produces very constant magnets, and I find from Prof. Thompson's table that its ratio of H_c to I_r is nearly the highest in the list. Even terra-cotta, which Prof. Thompson says may have a ratio exceeding that of any known brand of steel, has been found to preserve its magnetism for centuries, and is thus favourable to the view that a high ratio is indicative of high permanence. In short, the product $H_c \times I_r$ and the ratio $H_c \div I_r$ are not rival expressions for the same thing, but represent different quantities, and it is a mistake to confuse them. The product gives information regarding the magnitude of the hysteresis loop, the ratio

deals with its shape. The ratio measures the difficulty with which magnetism is removed ; it indicates the general slope of the demagnetisation curve, and is important when treating of the loss of magnetism as susceptibility is when treating of the growth of magnetism. I need hardly say I did not know that Mr. Matthews had very recently drawn attention to the use of the ratio H_c to I_r, and had proposed to call it " permanence." The coincidence with my suggestion is purely fortuitous, because, as my letter shows, the subject had been before me when carrying out experiments some years ago.

Prof. THOMPSON replied as follows : My sole point of view was the suitability of steel for making permanent magnets, for which purpose it is irrelevant to consider either soft iron or hard iron. I have pointed out in my lecture of 1912 that a certain tool steel having a coercive force of 85, even though it have a remanence of only 370, is better for short bar magnets than any other tungsten steel ; and, indeed, it is better for that purpose than any known kind of steel whatever, even though some have a remanence of 800 or more. It is of no value whatever for the permanence of a magnet's magnetism, whether measured by tractive power or by magnetic moment, that, with a given (high) coercive force, *the remanence should be low* ; but that misleading proposition is the logical meaning of the proposal to take as the criterion of permanence the *ratio* of coercive force to remanence. Stability depends essentially on the highness of the former—on its absolute magnitude, not on its magnitude relating to the latter.

XIV. *A Galvanic Cell which Reverses its Polarity when Illuminated.* By ALAN A. CAMPBELL SWINTON.

IN ordinary cells such as are used to show the alteration of the resistance in selenium under the action of light, the selenium is held between two metallic wires or strips, the light penetrates into the selenium at right angles to the direction of the electric current, and an exterior source of E.M.F. is employed.

The writer has constructed several selenium cells in which the selenium instead of being enclosed between two solids, as mentioned above, was contained between a solid and an electrolyte, when, as the latter was transparent, the light could be caused to pass through the selenium in the same or in the opposite direction to the electric current.

The selenium was spread upon a piece of copper which had previously been tinned to make the selenium more adherent, and then cooked for several hours in the usual manner, the uncovered portions of the copper being varnished with enamel, so that the electrolyte was not in contact with any part of the copper, but only with the selenium. To this end it was found important that there should be no cracks or pores in the selenium coating.

The other electrode consisted of a metal or carbon plate having a hole cut in it the same size as the selenium covering on the first plate, and the two plates were mounted parallel to one another in a coil with a glass window, so that light from the arc in a lantern could be projected through the window and the aperture in the metal or carbon plate on to the selenium surface.

Plain tap water was used as the electrolyte, any acid or alkali being apt to cause the selenium to come away from the copper. As in any case the electrical resistance of the selenium is very great, the extra resistance of the tap water was found of small account. It was found that such cells could be used in the ordinary way to show what is usually called the variation of the resistance of the selenium in light and in darkness with current derived from a separate battery. Some of the cells thus constructed gave good results, though not better than others constructed in the ordinary way.

If the perforated plate was made of zinc, the latter being electro-positive to selenium, the cell then worked as its own battery, the result of light falling on the selenium being to cause a larger flow of current. This might either be because the light caused the selenium to become more electro-negative, or because it reduced the resistance of the selenium. Subsequent tests, in which a Dolezalek quadrant electrometer was used instead of the galvanometer that had previously been employed, showed that the light caused an increase in the E.M.F.

If, however, instead of zinc, carbon or copper was employed for the perforated plate, we had the interesting result that while the selenium proved electro-positive to the carbon or copper in the dark, it immediately became electro-negative to carbon or copper the moment it was illuminated. The reversal of the polarity of a cell of this kind was easily shown on a D'Arsonval moving-coil galvanometer of some 285 ohms resistance, large and about equal deflections on the two sides of zero being observed in contrary directions when the light was turned on and off. There was rapid polarisation in either direction, but this polarisation was instantaneously destroyed by reversal of polarity by changing from light to darkness, or vice versa. Measurements were also made with the Dolezalek electrometer, and the E.M.F. proved to be from 0·06 to 0·11 volt in each direction. Tests made with a spectrum showed that the orange and red had the most effect, the violet appearing to have no effect at all. The results differ from those obtained by the late Prof. Minchin with his photo-electric cells of selenium and aluminium in methyl-alcohol and acetone ("Phil. Mag.," 5-31, pp. 229-234, 1891), inasmuch as these latter gave no current at all unless illuminated. Again, as in the writer's experiments both electrodes had approximately an equal amount of surface illuminated, the results seem to have no connection with those studied by Becquerel, who found that in an electrolytic cell a difference of potential can be obtained when one electrode is in darkness and the other is illuminated. Moreover, the effect with the selenium was found to be hundreds of times more powerful than what can be obtained by merely illuminating one of two similar electrodes and keeping the other in darkness.

The result with the selenium would appear to be consonant with the action of the light being to cause the emission from the selenium into the electrolyte of negative corpuscles. In

any case, the results point to the so-called alteration in the resistance of selenium under illumination being in reality due to an E.M.F. being generated.

The writer is much indebted to Mr. A. L. Davis for his skill and patience in constructing the cells and for his assistance in carrying out the investigations.

ABSTRACT.

If two plates—one of zinc and the other of tinned copper coated on one surface with selenium and varnished with enamel over the remainder of its surface—are immersed in tap-water, the electric current through a galvanometer connected to the plates shows that in the dark the zinc is electro-positive to the selenium, while the result of light falling on the selenium is to increase the effect.

If, however, instead of zinc, carbon or copper is employed for the non-coated plate, the interesting result is obtained that, while the selenium proves to be electro-positive to the carbon or copper in the dark, it immediately becomes electro-negative to carbon or copper the moment it is illuminated, this being easily shown by the deflections of the galvanometer in contrary directions as the light is turned on and off.

DISCUSSION.

Prof. T. MATHER asked if the increase of current produced by the illumination was proportional to the E.M.F. which the author had detected with the quadrant electrometer.

Mr. S. D. CHALMERS asked if heat waves would produce the effect.

Mr. W. DUDDELL commented on the fact that the visible rays were most effective while the ultra-violet rays were ineffective, and contrasted the result with the recent discovery that selenium is very sensitive to X-rays.

Mr. D. OWEN asked if the action was instantaneous. If so, by using intermittent illumination one should get a strong effect produced in a telephone circuit.

Mr. G. D. WEST thought that the ineffectiveness of the ultra-violet radiation might be due to the absorption of these rays which was bound to occur in the electrolyte.

The AUTHOR, in reply, said that the extreme instability of the cell made it impossible to co-ordinate readings of the current and the E.M.F. produced by illumination. He did not think heat waves were able to produce the effect, as passing the light through a solution of alum did not greatly reduce it. The action, he thought, was practically instantaneous. He agreed that the absorption of the cell would probably account for the ineffectiveness of the ultra-violet rays.

XV. *An Investigation of the Photographic Effect of Recoil Atoms.* By A. B. Wood, *M.Sc.,* and A. I. Steven, *M.A., B.Sc., University of Liverpool.*

Introduction.

MEASUREMENTS of the ranges of α-particles by different methods have shown that the ionising, phosphorescent and photographic actions cease after the α-particles have traversed the same distance in air, although they still possess nearly 40 per cent. of their initial energy. It seems reasonable to suppose, therefore, that these three properties of the α-rays must be ascribed to the same cause.

Work of Wertenstein* and one of us† has shown that the ionisation produced by a recoil atom is about 10 times greater over its range than that produced by the corresponding α-particle over the same part of the range.

It thus appears reasonable to expect that recoil atoms will produce phosphorescence and will affect a photographic plate. The object of experiments to be described was to investigate any photographic action if it occurs.

In the majority of cases the recoil atom is radio-active (*e.g.,* recoil atoms from RaA, RaC, ThC, Act.C), hence any photographic action due to energy of recoil is masked by the subsequent disintegration of the recoiling atom. Polonium is the ideal substance for the purpose of the experiments, since the atom recoiling from it, when it expels an α-particle, is itself rayless, and consequently any effect produced by it must be due entirely to the energy of its recoil.

EXPERIMENTAL PROCEDURE.

There still remains the separation of the effects due to the α-particles and those due to the recoil atoms. Two methods have been adopted :—

(A) By " absorbing " the recoil atoms.

(B) By deflecting the α-particles in a magnetic field.

* Wertenstein, " Comptes Rendus," 152, p. 1657, 1911.
† Wood, " Phil. Mag.," October, 1913.

(A.) *Absorption Method.*

In this method a very active preparation of polonium. deposited on a polished platinum surface. was placed at the end of a brass " light-tight " vessel. which could be evacuated to any desired pressure. The rays emitted from this source. after passing through two parallel slits 1 mm. wide and 1 cm. apart, fell on a sensitive photographic plate. one half of which was covered with tinfoil of thickness sufficient to prevent the α-rays from striking the film beneath it. The experiment now consisted of two distinct parts : (a) The pressure in the chamber was adjusted so that the recoil atoms were just prevented from reaching the plate. This pressure. a few centimetres. could easily be calculated from a knowledge of the range of recoil atoms at atmospheric pressure and the distance apart of the polonium source and the photographic plate. By means of a shutter arrangement an exposure of any required period could be given.

(b) The pressure was now reduced to about 0·001 mm. by means of a Gaede pump. until practically all the recoil atoms could reach the plate. the α-rays. of course. reaching it as before. Now. the other half of the plate was exposed for the same length of time. the part previously exposed being covered with tinfoil.

In this way one half of the plate was affected by α particles only, whilst the other half was affected by both α particles and recoil atoms. On developing the plate a narrow continuous band was observed across it, one half being produced in experiment (a). the other in (b). In nearly every case. that half of the band produced in experiment (b) was slightly darker than that produced in experiment (a).

It might appear from this that the effect of the α-particles and recoil atoms together was greater than the effect produced by the α-particles alone. thus showing that the recoil atoms have a photographic action. It must be considered. however. that in experiment (a). when the pressure in the chamber is several centimetres of mercury. fewer α-particles reach the plate on account of scattering than in experiment (b). when the pressure is extremely low. Consequently. the slight difference of intensity observed might be due to this absorption or scattering effect alone. Hence it became necessary to adopt some other method by which this effect could be eliminated.

(B.) *Magnetic Deflection Method.*

Makower and Evans* have shown in the case of RaB recoiling from RaA that the deflection of recoil atoms in a magnetic field is only half that of the corresponding α-particles. Assuming that this is the case for other substances emitting α particles, we have here a convenient method of separating the two effects. The apparatus used is essentially similar to that employed by Makower and Evans† in the determination of "e/m" and "v" for the atoms recoiling from RaA, the only difference being that in our experiments the recoil atoms were received directly by a photographic plate. A powerful electromagnet, giving a field of about 10,000 gauss with pole-faces 3·5 cm.×1 cm. and a gap 1 cm., was employed to deflect the beam of rays issuing through the slits. Equal exposures were made with the field first in one direction, then reversed, the chamber being evacuated to a very low pressure by means of a Gaede pump.

If both α-particles and recoil atoms affect a photographic plate, then we should expect to find, on developing the plate, four lines, the two outer ones being due to α-particles and the two inner ones being the result of the impact of recoil atoms. Even with the longest exposures possible, limited by the heating of the magnet, only two lines about 1mm. wide and 0·4 cm. apart (obviously due to α-particles) were observed.

Now, Wertenstein‡ and one of us§ have shown that the penetrating power of recoil atoms is only about $\frac{1}{500}$th that of the α-particles, consequently the negative result just mentioned might be explained on these grounds—*i.e.*, the recoil atoms do not penetrate sufficiently deep into the film to produce an observable effect. To get over this difficulty, instead of using ordinary gelatine-coated plates, Schumann plates (a thin film of silver chloride on glass) were employed ; the recoil atoms had thus a greater advantage in affecting the sensitive layer. Many experiments were made using these plates, but the results were either entirely negative or so uncertain as to be quite useless.

* Makower and Evans, " Phil. Mag.," Vol. CXIX., November, 1910, p. 882.
† Makower and Evans, *loc. cit.*
‡ Wertenstein, *loc. cit.*
§ Wood, *loc. cit.*

Summary and Discussion of Results.

An attempt has been made by two distinct methods to
demonstrate the photographic action of recoil atoms from
polonium. The method of separating the α-particles from the
recoil atoms by means of a magnetic field is undoubtedly the
more conclusive of the two methods employed in this investi-
gation ; but, with the sources of polonium at our disposal,
the results show that the photographic effect, if any, of the
recoil atoms is too small to be observed. This negative result
may possibly be due to either or both of two causes :—

1. The activity of the polonium source used was not suffi-
ciently great ;

2. The recoil atoms do not penetrate sufficiently deep into the
sensitive layer to produce an appreciable photographic effect.

By employing a source of very great activity both these
difficulties might be overcome and a more conclusive result
obtained.

It might be mentioned here that one of us has carried out a
series of experiments on the possibility of recoil atoms pro-
ducing scintillations on a zinc sulphide screen, but the results
in these experiments also were inconclusive.

Our best thanks are due to Prof. Sir E. Rutherford for the
loan of the source of polonium, and to Prof. L. R. Wilberforce
for his kind interest in the experiments.

ABSTRACT.

The ionising, phosphorescent and photographic effects of the
α-particles from a radio-active substance entirely cease when the
particle still retains about 40 per cent. of its kinetic energy. It
appears possible, therefore, that the recoil atoms from a radio-
active source should be able to affect a photographic plate, for though
the range of a recoil atom is only about $\frac{1}{30}$th of that of the α-particle
shot off from it, the ionising effect has been shown by Wertenstein
and one of the authors to be 10 times as powerful over the corre-
sponding range as that of the α-particle.

Attempts have, therefore, been made to demonstrate this action
in the case of the recoil atom from polonium, this substance being
chosen on account of the inactive nature of the recoiling atom. Two
distinct methods were employed : (1) The recoil atom was " ab-
sorbed " ; (2) the difference of deflection of the α-particle and the
recoil atom in a strong magnetic field was utilised in order to attempt
to separate their effects. " Schumann " plates were used as being
most easily penetrable, but in all cases the results were negative or
inconclusive. This is probably due to the fact that the recoil atoms
are not able to penetrate sufficiently deeply into the sensitive layer
to render the grains developable.

XVI. *Magnetic " Character " Figures: Antarctic and International.*   *By* C. Chree. *Sc.D., LL.D., F.R.S.*

§ 1. Since January, 1906, an international scheme has existed for assigning to individual days magnetic " character " figures " 0 " (quiet), " 1 " (moderately disturbed), " 2 " (highly disturbed). These figures have been assigned independently at each co-operating observatory, the observer taking into account only the curves of his own station. The results are sent in quarterly to the Director of De Bilt Observatory in the Netherlands, who undertakes the analysis and publication. At the year's end he issues a list ascribing to each day a " character " figure which represents the mean estimate of the co-operating stations, the results being given to 0·1. On these figures he bases the selection for each month of five " quiet " days, which are recommended for the deduction of the regular diurnal variation for international purposes. A " day " in this connection means a period of 24 hours commencing at Greenwich midnight.

For purposes of my own I have utilised the international data for the selection of the five most disturbed days of each month, regarded as the days having the largest " character " figures. Occasionally the question arises which of two days having equal " character " figures should occupy the fifth place. In such a case preference has been given to a day immediately adjacent to a highly disturbed day.

The impression produced on the mind by a magnetic trace depends a good deal on the sensitiveness of the magnetograph. With very high sensitiveness, such as 1 mm. $= 1\gamma (1 \times 10^{-5}$ C.G.S.) disturbances catch the eye which on an insensitive record, such as 1 mm. $= 10\gamma$, seem insignificant. Some stations, again, are much more disturbed than others, while different observers have different ideals. Thus the standard differs at different observatories. What one observer accepts at once as a " 2 " day, another equally readily accepts as a " 1 ". Further, as I have shown elsewhere,* one's estimate of disturbance is apt to be influenced by the presence or absence of large disturbances about the time. If one maintained an absolutely uniform standard, class " 0 " would be but slightly represented

* Roy. Soc., " Phil. Trans.," A.   Vol. 213, p. 245.

in a disturbed season, and class " 2 " in a quiet season.   Thus even the final international figures do not, I think, have an exact physical significance, but they do at least discriminate with very considerable accuracy between the days of the same month.

The large majority of the stations contributing to the international lists lie between 55 N. and 25 N. lat.   The extreme latitudes represented are 59·7 N. (Pavlovsk) and 31·7 S. (Pilar). There are observatories further south than Pilar in British territory from which figures would be valuable.

There is a great difference magnetically between stations in moderate and high latitudes.   At all stations, so far as I am aware, which have been occupied within the Arctic or Antarctic circle, magnetic disturbances have shown a persistence and amplitude unparalleled in temperate or torrid zones.   There are grounds for believing a connection between magnetic disturbances and auroras of a rapidly oscillating type.   The frequency of aurora in Europe diminishes rapidly as one moves southwards from the auroral belt.   Auroras are frequent occurrences as far south as Shetland, but very rare in southern England.   According to Prof. K1. Birkeland's* theory, which has a good deal to say for itself, auroras and magnetic storms are alike due to flights of electric ions discharged from the sun. When these ions enter the earth's magnetic field they describe spirals round the lines of force, and none which do not possess an exceptionally high velocity, closely approaching that of light, can get near the earth, except within a moderate angular distance of the magnetic poles.   There may thus be an almost continuous advent of ions with consequent aurora in high latitudes, and but very occasional advent in temperate latitudes.   The disturbance due to the ions present in high latitudes is, of course, not *wholly* limited to these latitudes, but only faint effects would ordinarily be experienced in remote regions.   On this view there need be no close connection between the prevalence of disturbance in mid-Europe and in high latitudes, especially high southern latitudes.   Unless on the relatively few occasions when numerous ions of high velocity are discharged from the sun, there are also unusually copious discharges of the slower moving ions, the presence of a magnetic storm in central and southern Europe would not necessarily

* " Expédition Norvégienne, 1899-1900."   " The Norwegian Aurora-Polaris Expedition, 1902-1903," Vol. I., Sects. I. & II.   Also numerous Papers in the " Comptes Rendus " of the French Academy since 1908.

imply any special disturbance in the Arctic and Antarctic. Conversely, the presence of quiet conditions in mid-Europe would raise but a slight presumption of quiet conditions in high latitudes. Thus theoretical as well as practical considerations indicated the desirability of examining the records obtained during Captain Scott's last Antarctic Expedition, to see whether there is any marked connection between the magnetic " character " of days in high and moderate latitudes. It was also investigated whether the so-called 27-day period manifests itself in the Antarctic.

§ 2. In previous investigations* of the 27-day period I have employed two sets of magnetic data, viz., the absolute daily ranges of the magnetic elements and " character " figures, either the international figures published at De Bilt, or figures assigned by myself to Kew curves, for years for which no international data exist. Absolute daily ranges have been got out for the Antarctic curves, but they refer to days in the local time of 180° longitude, and the additional labour necessary to determine a second set of ranges applicable to days G.M.T. hardly seemed justified. Thus I decided to assign and employ " character " figures.

Thanks to the assiduous care of the Antarctic magnetic observers, Dr. G. C. Simpson and Mr. C. S. Wright, there are but few gaps in the record extending from the beginning of February, 1911, to near the end of November, 1912. There were, however, a few days on which the loss of trace rendered it impossible to assign a " character " figure satisfactorily. A few hours' trace may justify one in assigning a " 2," but unless the day's trace is very nearly complete it is hardly safe to assign a " 0."

Table I. gives the dates of the five most disturbed and the five international quiet days of each month, along with the corresponding " character " figures assigned by myself to the Antarctic curves. A few days are left blank for the reason stated above. The " character " figures are taken from a list including all days for which adequate Antarctic records existed. When making the estimate I did not consult the international list, so there was no possible prejudice. Before actually assigning the figures, I made a general inspection of all the Antarctic curves, so as to set myself a standard of disturbance likely to discriminate adequately between the days of all the

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* Roy. Soc. " Trans." A.   Vol. 212, p. 75 ;   Vol. 213, p. 245 ;   " Proc." A, Vol. 90, p. 583.

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months. If I had applied such a standard as is applied to Kew curves, most of the summer months would have received no 0's and very few 1's. Even as it is, the number of 2's allotted is much greater than at the average station co-operating in the international scheme.

TABLE I.—*Antarctic "Character" Figures on International Disturbed and Quiet Days.*

Dates.											Antarctic character figures.									
1911.	Disturbed days.					Quiet days.					Disturbed days.					Quiet days.				
February	2	13	21	22	23	11	12	15	19	20	2	2	2	2	2	1	1	1	1	0
March	20	21	23	25	26	10	11	12	17	18	2	2	2	2	2	0	0	0	1	0
April	8	9	16	17	30	5	13	14	15	26	2	2	2	2	2	0	−	0	1	1
May	7	14	15	16	31	1	4	13	22	24	2	2	2	2	2	0	0	0	1	0
June	4	5	9	10	11	3	17	18	19	25	1	2	1	2	1	1	0	0	0	0
July	1	17	19	28	29	13	14	15	16	26	2	1	2	2	2	1	1	0	1	0
August	19	23	24	25	26	7	8	10	11	29	2	2	2	2	2	1	0	1	0	1
September .	16	19	20	21	22	2	3	14	25	26	2	1	2	2	2	1	0	1	0	1
October	9	10	11	17	18	1	5	15	23	28	2	2	2	2	2	0	0	2	1	1
November...	3	9	13	14	15	1	7	22	23	24	2	2	2	2	2	1	1	1	1	1
December...	6	11	17	26	31	2	9	21	22	23	2	2	2	2	2	2	0	2	1	2
1912.																				
January	11	12	13	17	22	2	15	16	26	27	1	2	2	2	2	1	1	1	1	0
February ...	12	13	16	17	26	5	6	15	20	21	2	1	1	2	−	0	1	1	1	0
March	7	8	9	21	29	4	17	18	19	24	1	2	1	1	1	0	0	0	0	1
April	5	6	10	15	16	1	8	11	21	28	2	2	2	2	2	0	0	0	0	0
May	5	6	12	13	14	1	16	22	23	26	2	1	2	2	2	1	0	0	0	1
June	1	8	9	10	28	5	6	15	19	20	2	2	2	2	1	0	1	0	0	0
July	3	4	5	27	31	10	11	12	15	24	1	2	2	1	1	0	0	0	0	0
August	5	6	18	19	22	4	8	12	13	26	1	1	3	2	2	0	0	0	0	1
September .	4	17	18	23	24	2	15	16	27	28	1	2	3	1	2	0	0	1	0	0
October	1	11	13	14	15	2	5	18	19	31	2	2	1	2	2	1	1	1	0	1
November...	10	11	14	16	22	3	12	21	29	30	2	2	−	2	−	1	1	−	−	−

A glance at Table I. shows that the association between Antarctic disturbances and those in moderate latitudes was very intimate. Of the 107 international highly disturbed days for which Antarctic records existed, 85 got a "2," and not a single one a "0." Only one day which had an international "character" figure as large as 1·3 failed to get a "2," and it was a day on which several hours' trace was missing, so that allotting it a "1," as I did, was rather a rash proceeding. On the other hand, 57 of the 106 international "quiet" days got a "0" and only four got a "2." Of these, three occurred in December, 1911, a month to which I allotted sixteen 2's and only one 0. Moreover, all three were essentially quiet except for a few hours near midnight, and the disturbance was so moderate that it was open to doubt whether a "1" or a "2" was fairer.

The fourth international quiet day which got a " 2 " was also close to the border line between " 1 " and " 2." In only one of the four cases was there any suggestion from the international data that there had been a disturbance peculiar to high latitudes. On that occasion a " 1 " had been allotted at Pilar, the most southern station, and at Sitka, one of the most northern, every other station with one exception assigning a " 0."

§ 3. Various other interesting conclusions could be derived from Table I., but they are more readily recognised in Table II., which analyses the data from the different months. The Antarctic selected disturbed and quiet day data for November, 1912, were derived from only three disturbed and two quiet days, and only 15 days were available altogether, so the results for that month are somewhat uncertain.

TABLE II.—*Mean Monthly and Annual "Character" Figures.*

	1911.						1912.					
	International.			Antarctic.			International.			Antarctic.		
—	Dist.	All.	Quiet.	Dist.	All.	Quiet.	Dist.	All.	Quiet.	Dist.	All.	Quiet.
January	...	...	...	...	...	...	0·98	0·42	0·02	1·8	1·26	0·8
February ...	1·46	0·89	0·36	2·0	1·54	0·8	1·02	0·49	0·12	1·5	1·00	0·6
March	1·48	0·78	0·08	2·0	1·26	0·2	1·08	0·45	0·02	1·2	0·83	0·2
April	1·48	0·76	0·14	2·0	1·28	0·5	1·12	0·45	0·08	2·0	0·73	0·0
May	1·28	0·70	0·16	2·0	1·23	0·2	1·18	0·47	0·10	1·8	0·77	0·4
June	1·12	0·53	0·04	1·4	1·00	0·2	0·98	0·47	0·08	1·8	0·86	0·2
July	1·26	0·61	0·12	1·8	1·32	0·6	1·06	0·41	0·02	1·4	0·70	0·0
August	1·30	0·53	0·10	2·0	1·06	0·6	1·12	0·49	0·02	1·6	1·03	0·2
September .	1·30	0·50	0·06	1·8	0·97	0·6	1·22	0·47	0·02	1·6	0·97	0·2
October	1·30	0·59	0·06	2·0	1·26	0·8	1·20	·046	0·02	1·8	1·23	0·8
November...	1·42	0·49	0·04	2·0	1·47	1·0	1·06	0·45	0·00	2·0	1·53	1·0
December .	1·30	0·45	0·08	2·0	1·48	1·4	...	...	...	...	...	...
Means	1·34	0·62	0·11	1·91	1·26	0·63	1·09	0·46	0·04	1·68	0·99	0·40

As already indicated, the necessity of discriminating adequately between the different days of the same month tends to variation in the international standard, of such a nature as to minimise the difference between different years. The fall in disturbance, however, between 1911 and 1912 was so large that it is readily recognised in the international figures in Table II., whether one regards the disturbed day, the quiet day, or the all-day results. There is also, it will be seen, a marked decline in the Antarctic " character " figures. Thus in this case, at least, the fall in disturbance was common to high and

temperate latitudes. According to statistics advanced by some authorities, auroras have been more numerous at stations to the north of the Arctic aurora belt during years in which they have been less numerous at stations to the south of it. Thus it would not have been safe to assume *a priori* that disturbances near a magnetic pole and those in temperate latitudes wax and wane together.

Another phenomenon clearly visible in Table II. seems peculiar to the Antarctic. In temperate latitudes disturbance is most prominent towards the equinoxes. In the Antarctic the equinoctial months, while decidedly more disturbed than the winter months, are less disturbed than the summer months. This phenomenon was also clearly shown by the magnetic curves obtained in 1902 and 1903 by the first Scott Antarctic Expedition.

TABLE III.—*Antarctic " Character " Figures. Sums on Selected and Associated Days.*

	$n-2$	$n-1$	n	$n+1$	$n+2$	$n+25$	$n+26$	$n+27$	$n+28$	$n+29$	$n+30$
Disturbed and associated days:											
1911 (55 days)	55	76	106	98	88	63	72	83	90	85	76
1912 (49 „)	44	58	81	72	53	48	55	64	57	56	45
Total (104 days)	99	134	187	170	141	111	127	147	147	141	121
Quiet and Associated days:											
1911 (55 days)	67	54	35	46	66	65	50	47	54	67	74
1912 (49 „)	42	35	16	31	44	37	41	41	44	49	50
Total (104 days)	109	89	51	77	110	102	91	88	98	116	124
Disturbed and associated, less quiet and associated:											
1911 (55 days)	−12	+22	+71	+52	+22	−2	+22	36	+36	18	+2
1912 (49 „)	+2	+23	+65	+41	+9	+11	+14	23	+13	+7	−5
Total (104 days)	−10	+45	+136	93	+31	+9	36	+59	−49	+25	−3

§ 4. In order to ascertain whether the 27-day period is recognisable in the Antarctic, the " character " figures which had been assigned were entered in tables of which Table III. constitutes a summary, in columns headed $n-2$ to $n+2$ and $n+25$ to $n+30$. Here n stands for a representative selected day, whether one of the five disturbed or one of the five international quiet days of the month, $n-1$ and $n+1$ represent the days immediately preceding and succeeding. $n+25$ the day 25

days later, and so on.   None of the selected days for November,
1912, appeared in column n because there were no correspond-
ing data for columns $n+25$ to $n+30$.   What appears in Table
III. is the *sum* of the " character" figures, allowance being made
for one or more days missing in some of the columns.   For
instance, in 1912, in the case of the disturbed and associated
days, only 48 days' " character" figures were really available
for column $n+2$.   Their sum 52 was brought up to 53, as the
integer nearest to $(49/48) \times 52$.

The 27-day period is clearly shown in the case alike of the
disturbed and the quiet days, and still more clearly in the
difference figures, obtained by substracting the totals for the
quiet and associated days from the corresponding totals for the
disturbed and associated days

A similar procedure was applied to days that were from 30
to 25 days earlier than the selected disturbed and quiet days.
In this case the selected days of February, 1911, could not be
utilised.   Table IV. gives the results obtained for the columns
$n-30$ to $n-25$.   The results for the columns $n-2$ to $n+2$
differed so little from the analogous results in Table III. that it
seemed unnecessary to give them.   While Table III. suggests a
period distinctly longer than 27 days, Table IV. would make the
period more nearly 26 days.   This is not improbably connected
with the lack of symmetry which Table III. discloses in the
primary pulse.   The " character" sum in column $n+1$ is
greater for disturbed and less for quiet days than the corre-
sponding " character" sum in column $n-1$.   Thus the centre
of the primary pulse would seem to fall well after the middle of
the representative selected day.

TABLE IV.—*Antarctic " Character" Figures.   Sums on Associated Days.*

	$n-30$	$n-29$	$n-28$	$n-27$	$n-26$	$n-25$
Disturbed associated days:						
1911 (50 days)	49 ·	51	67	79	84	80
1912 (55 ,,)	61	56	55	67	70	67
Total (105 days)	110	107	122	146	154	147
Quiet associated days:						
1911 (50 days)	64	60	54	48	47	51
1912 (55 ,,)	57	51	47	49	49	47
Total (105 days)	121	111	101	97	96	98
Disturbed associated less quiet associated (105 days)	−11	− 4	+21	+49	+58	+49

Table V. employs the same data as Table III., but arranges them under three seasons, viz., winter (May to August), summer (November to February) and equinox (the remaining four months). The 27 day period is apparent in each season, but is less well marked in summer than in winter or equinox. This is partly explained by the smaller number of days available in summer, and partly by the fact that disturbances at that season were so large and numerous that the number of "character" 0's allotted was hardly sufficient to discriminate adequately between the days of the same month.

The sun, it should be remarked, was continuously above the horizon almost the whole of summer, and continuously below the horizon almost the whole of winter. Its being below the horizon during the latter season seems not to have militated in any way against the 27-day period.

TABLE V.—*Antarctic " Character " Figures. Same as Selected and Associated Days.*

		$n-2$	$n-1$	n	$n+1$	$n+2$	$n+25$	$n+26$	$n+27$	$n+28$	$n+29$	$n+30$
Winter,	Dist., &c.	37	50	69	64	51	40	49	57	57	51	41
40 days	Quiet, &c.	34	25	12	23	31	34	26	25	34	37	39
Excess of dist'bd, &c.		+ 3	+25	+57	+41	+20	+ 6	+23	+32	+23	+14	+ 2
Equinox,	Dist., &c.	36	50	72	64	57	45	48	39	59	61	53
40 days	Quiet, &c.	42	34	16	25	42	40	39	39	42	51	53
Excess of dist'bd, &c.		− 6	+16	+56	+39	+15	+ 5	+ 9	+20	+17	+10	0
Summer,	Dist., &c.	27	35	47	44	34	27	31	32	32	30	28
25 days	Quiet, &c.	34	31	23	30	38	29	27	25	23	29	33
Excess of dist'bd, &c.		− 7	+ 4	+24	+14	− 4	− 2	+ 4	+ 7	+ 9	+ 1	− 5

§ 5. The mean of the Antarctic " character " figures, notwithstanding the much higher standard of disturbance employed, was much in excess of the corresponding international mean. Thus it appeared desirable for purposes of comparison to express the results as percentages of some standard value. Two quantities suggested themselves as standards, 1° the mean " character " figure from all days of the year, and 2° the amplitude of the primary pulse, i.e., the excess of the " character " figure of the representative disturbed day over that of the representative quiet day of the year. Both have been used. The Antarctic data referred to both standards are given in Table VI. The international data referred to the two standards appear separately in Tables VII. and VIII. These two last

tables are not confined to 1911 and 1912, but include all the years for which international data have been published.

To explain the figures, consider the Antarctic results for 1911 in Table VI. The mean " character " figure for 1911 was 1·26. There were 55 selected disturbed and 55 selected quiet days. The sum of the " character " figures for 55 average days is $55 \times 1·26$ or 69·3. The sums for the 55 selected days were respectively : disturbed 106, quiet 35. The difference of these sums—or amplitude of the primary pulse—71, expressed as a percentage of 69·3, is to the nearest integer 102. The sums for the 55 days which followed 27 days after the selected days were respectively : associated disturbed days 83, associated quiet days 47, and again to the nearest integer $100 (83—47)/69·3 = 52$.

The figures in the second line of Table VI. were got in an exactly similar way, using as standard 0·99, the mean " character " figure for 1912.

TABLE VI.—*Antarctic " Character " Figures. Differences Disturbed and Associated Days less Quiet and Associated Days as percentages.*

		$n-2$	$n-1$	n	$n+1$	$n+2$	$n+25$	$n+26$	$n+27$	$n+28$	$n+29$	$n+30$
As percentages of mean character figure for year	1911	−17	+32	+102	+75	+32	− 3	+32	+52	+52	+26	+ 3
	1912	+ 4	+47	+134	+85	+19	+23	+29	+47	+27	+14	−10
	Mean	− 7	+40	+118	+80	+25	+10	+30	+50	+39	+20	− 4
As percentages of difference dist. less quiet on day n	1911	−17	+31	+100	+73	+31	− 3	+31	+51	+51	+25	+ 3
	1912	+ 3	+35	+100	+63	+14	+17	+21	+35	+20	+11	− 8
	Mean	− 7	+33	+100	+68	+22	+ 7	+26	+43	+35	+18	− 3

TABLE VII.—*International " Character " Figures. Differences Disturbed and Associated Days less Quiet and Associated Days as percentages of Mean " Character " Figure for Year.*

Year.	$n-2$	$n-1$	n	$n+1$	$n+2$	$n+25$	$n+26$	$n+27$	$n+28$	$n+29$	$n+30$
1906	22	92	179	96	15	6	37	47	24	7	−16
1907	17	96	179	83	2	8	31	31	17	18	29
1908	14	94	175	98	17	27	60	57	44	15	− 2
1909	29	94	195	103	25	−15	0	37	41	35	17
1910	21	83	161	80	18	5	37	56	47	22	6
1911	10	86	189	93	16	26	65	89	69	21	2
1912	0	98	233	109	−18	17	48	69	18	−14	−16
1913	52	131	228	118	40	59	88	98	62	46	34
Mean	20·6	96·7	192·5	97·5	14·5	16·7	45·7	60·4	40·3	18·7	6·8

TABLE VIII.—*International " Character" Figures. Differences Disturbed and Associated Days less Quiet and Associated Days as percentages of Difference on Day n.*

Year.	$n-2$	$n-1$	n	$n+1$	$n+2$	$n+25$	$n+26$	$n+27$	$n+28$	$n+29$	$n+30$
1906	12	51	100	54	9	4	20	26	14	4	− 9
1907	10	53	100	46	1	4	17	17	10	10	16
1908	8	53	100	56	10	15	34	33	25	8	− 1
1909	15	48	100	53	13	− 7	0	19	21	18	9
1910	13	52	100	49	11	3	23	34	29	13	4
1911	5	46	100	49	9	14	35	47	36	11	1
1912	0	42	100	47	− 8	7	21	30	8	− 6	− 7
1913	23	57	100	52	18	26	39	43	27	20	15
Mean	10·7	50·4	100	50·8	7·7	8·2	23·6	31·1	21·2	9·9	3·5

As regards the second set of percentages in Table VI., we have already seen that the amplitude of the primary pulse for 1911 was 106—35, or 71. In column $n+27$, for example, for 1911 we have 100(83—47) 71=51 to the nearest integer. The percentage figures for the individual years in Tables VI., VII. and VIII. were all calculated to one figure beyond that recorded, and these more exact figures were employed in deducing the final means given in the tables.

Fig. 1 shows separately the primary and secondary pulses for disturbed and associated and for quiet and associated days, the numerical data represented being the percentages whose difference appears in the last line of Table VII.

Fig. 2 shows the primary and secondary difference pulses derived from the Antarctic data in the last line of Table VI. and from international "character" data in Table VIII. The sunspot maximum group of years 1906, 1907 and 1908 had a mean sunspot frequency of 53·8, while the sunspot minimum group 1911, 1912 and 1913 had a frequency of 3·5.

The numerals attached to the observational points in both figures denote the number of days from the crest (marked 0) of the primary pulse, time previous being measured to the left and time subsequent to the right. In Fig. 2 each crest 0 is at the same height above its zero line, and corresponding primary and secondary pulses and their common zero line are represented by a common type of line.

Tables VII. and VIII. show that even when data from a large number of stations are combined, the prominence of the 27-day period varies a good deal from year to year. Data from a single station are naturally exposed to greater uncertainties, but it seems a pretty safe inference from Table VI. and Fig. 2, that the 27-day period is as prominent in the Antarctic as elsewhere.

Either set of results in Table VI. makes the 27-day period more prominent in 1911 than in 1912, agreeing in this with the international figures. This is one of the reasons for including other years in Tables VII. and VIII. In the absence of evidence to the contrary, the lesser development of the 27-day period in 1912
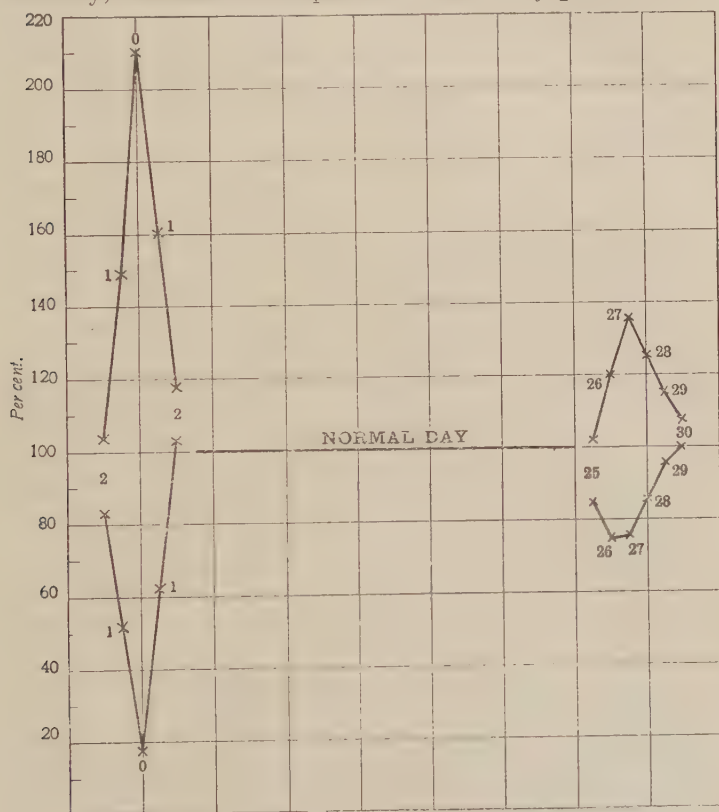

FIG. 1.—27-DAY PERIOD IN INTERNATIONAL "CHARACTER" FIGURES
DISTURBED AND QUIET DAY PULSES.

might have been ascribed to the fact that that year was near sunspot minimum. On Prof. Birkeland's* theory this would seem a very natural inference. It appears, however, from Tables VII. and VIII. that the period was more developed in

* In some of his writings on the subject Prof. Birkeland seems to identify sunspots with the sources of the electrical discharges to which he ascribes magnetic storms. In others, e.g., "The Norwegian Aurora Polaris Expedition, 1902-1903," Vol. I., Sect. II., p. 525, he seems to go no further than the conclusion "that sun spots and magnetic storms are both of them manifestations of the same primary cause."

1912 than in either 1907 or 1909, the former a year of sunspot maximum, while less developed than in 1913 which had even fewer sunspots. It is also more prominent in the curve for
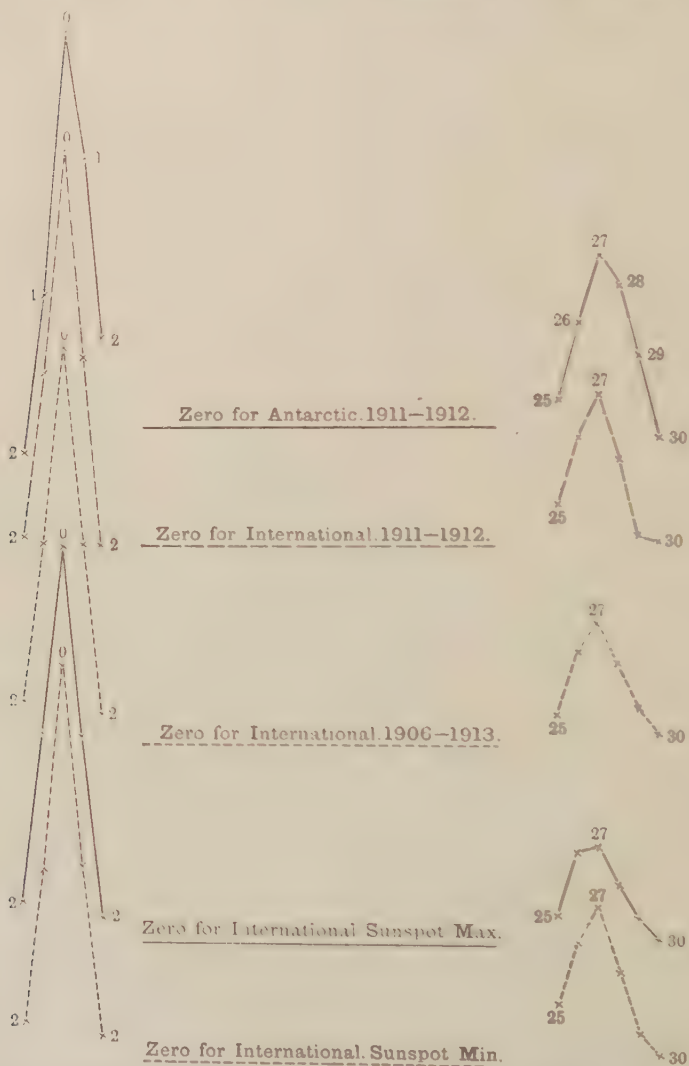

FIG. 2.—THE "27-DAY" PERIOD. "CHARACTER" FIGURES (DIFFERENCES):

sunspot minimum years than in that for sunspot maximum years in Fig. 2.

§ 6. The question seemed of sufficient interest to call for a further investigation, the results of which appear in Table IX. This gives the number of occasions on which a 27-day interval intervened between selected days, disturbed and quiet. The last column gives Wolfer's sunspot frequency. The frequency assigned to 1913 is still only " provisional," but Wolfer's provisional values seldom differ much from his final values.*

Antecedents to selected days in January, 1906, fell outside the range of international figures, thus 95 months only were available. If five random days had been selected from each month the probable number of sequences of any one species would have been 78, or roughly 10 a year. Thus the number of disturbed-disturbed sequences (*i.e.*, the number of occasions on which an interval of 27 days presented itself between two selected disturbed days) is nearly double the mathematical expectation. While these sequences are well represented in 1907, the year of sunspot maximum, they are even more numerous in 1911 and 1913 when sunspots were nearly absent.

The quiet-quiet day sequences exceed the disturbed-disturbed in four years out of the eight, but on the whole are distinctly less numerous. They are especially prominent in 1906 and 1910, years neither of sunspot maximum nor minimum. Their aggregate is no less than three times that of the disturbed-quiet sequences. The disturbed-quiet and quiet-disturbed sequences are practically equal in number.

Table IX. agrees with Tables VII. and VIII. in showing no special development of the 27-day period in years of many sunspots.

TABLE IX.—*Number of 27-day Sequences.*

First day......	Disturbed.	Quiet.	Disturbed.	Quiet.	Wolfer's sunspot
Second day...	Disturbed.	Quiet.	Quiet.	Disturbed.	frequency.
Year.					
1906	13	25	6	4	53·8
1907	19	10	9	4	62·0
1908	17	18	4	2	48·5
1909	15	17	8	11	43·9
1910	17	21	5	5	18·6
1911	25	18	5	5	5·7
1912	18	8	5	9	3·6
1913	28	15	2	3	1·2
Totals	152	132	44	43	...

* Since this was written Wolfer's final value has been published. This entails the substitution of 1·4 for 1·2 in the value for 1913 in Table IX., and of 3·6 for 3·5 in the mean value for the sunspot minimum years 1911, 1912 and 1913.

ABSTRACT.

The Paper makes use of magnetic " character " figures " 0 " (quiet day), " 1 " (moderately disturbed day), " 2 " (highly disturbed day) to investigate whether the incidence of disturbance at the base station of the Scott Antarctic Expedition, 1911-1912, did or did not accord with the incidence of disturbance in temperate latitudes : also whether the " 27-day period " could be recognised in the Antarctic data.

A very complete set of magnetic curves was obtained by the physical observers of the Antarctic Expedition, Dr. G. C. Simpson and Mr. C. S. Wright, extending from February, 1911, to November, 1912. " Character " figures were assigned to each day's records by the author, and a comparison was made with the corresponding international figures published annually at De Bilt, Netherlands.

The incidence of disturbance in the Antarctic was found to agree closely with that shown by the international lists, in spite of the fact that the disturbances in the Antarctic were much larger and more persistent than at any of the stations co-operating in the international scheme.

The " 27-day period " was clearly visible in the Antarctic records both in summer and winter, being as well developed there as elsewhere.

DISCUSSION.

Mr. DUDDELL thought the Paper very interesting. It was only by patient investigation of this kind that we should get any nearer a comprehensive theory of terrestrial magnetism.

Mr. F. E. SMITH remarked that it was described as " clearly visible " from Table II, that the disturbances were more pronounced in temperate latitudes during the equinoctial periods than in the intervening months. This was hardly obvious to the casual observer.

Prof. J. W. NICHOLSON said that Dr. Chree's Paper made the position of Birkeland's theory more curious than ever. Leaving out of consideration, as indisputable, the mathematical difficulties of the theory which Dr. Chree had referred to, he would like to indicate one point in which the theory had some support. If we were receiving particles from the sun, some might come from the corona, and might show themselves in the coronal spectrum and, later, in that of the aurora. There was, in fact, a strong correspondence between these spectra in some important particulars, and it does seem probable that we receive such particles. It is possible that the corona actually consists of the particles which are being shot from the sun and which we receive. But the spectra of sunspots are not in accordance with the view that we receive an unusual number from the spots.

Prof. O. W. RICHARDSON thought that in the case of particles streaming through the corona the auroral spectrum would be that of the atmosphere, and not that of the exciting particles. Only electrons should really be effective in reaching the earth under the proper conditions for the aurora.

Prof. NICHOLSON said that the correspondence in spectra would only be expected if the emitted particles were the corona itself, and not merely something passing through it. We do seem, as far as the evidence is trustworthy—the auroral spectrum being somewhat unreliable—to be receiving particles heavier than electrons, whether these are really responsible for the aurora or not.

Dr. A. RUSSELL said that in the Antarctic the disturbances seemed

less violent in winter than in summer, and it would be of interest to know if a similar relation held in the Arctic.

Dr. C. Chree, in reply, said that it should be remembered that in the Antarctic the midsummer months were December, January and February. The values for these months stand out more highly. Birkeland is not definite on the question as to whether sunspots are the origin of the ions or not. One reason for associating them with sunspots is that in sunspot minima the magnetic disturbances are not pronounced, but what one fails to find is an equivalent excess of disturbance during sunspot maxima.

XVII. *The Electrification of Surfaces as Affected by Heat. By*
P. E. SHAW, *D.Sc.*

Introductory.

THE uncertainty, as to sign, of the charge produced on unlike
solids when rubbed together has long been known. Thus, it is
possible to select three glass rods of different materials, A, B, C.
A rubbed with B becomes +, but when rubbed with C
becomes —. Again, most kinds of glass are — to flannel, but
some are +. Such different action is attributable to different
surface-hardness and to composition. Faraday knew of such
anomalies and mentioned some in his researches.

The particular irregularity with which this Paper deals is
that produced if the surfaces about to be rubbed are heated.
Thus, when smooth glass is rubbed with silk we have, as shown
by the action of the gold-leaf electroscope, glass+ silk —. Call
this the *normal* action.

If the glass be passed to and fro in a flame for a few seconds
we find on rubbing glass — silk —. Call this the *abnormal*
action. After finding this effect, I searched in several text-
books and treatises (including French and German), but found
mention of the phenomenon in one book only—viz., Hadley's
" Magnetism and Electricity," p. 149 (MacMillan). The sug-
gestion is thrown out there that it may be " due to the removal
by the flame of the film of air condensed on the surface of the
glass." There seems no definite reason for adopting this
theory of the action, and the following experiments appear to
disprove it. It is also mentioned in the above passage that
the anomalous effect may be removed by allowing the glass
to cool and then warming it again in a sand oven. I have tried
this method of obliteration, and have found it fail both when
the glass is resting on the hot sand and when it is buried in the
sand. See also Experiment (9) below. But it may answer
for one kind of glass and not for others. Two methods I have
found unfailing to bring the glass back to normal : (1) Long
continued rubbing with silk or cotton ; (2) passing the rod of
glass through the hand or a sheet of indiarubber. This is much
quicker in action than (1).

If a systematic examination of the common hard solids and

rubbing materials be made the abnormal effect will be found universal, though in varying degree.

The electrification series may be written :—

$+$ (Vitreous silica)	(Vulcanised fibre)	Metals	Sealing wax
Catskin	(Mica)	(Slate)	Resin
Flannel	Cotton	(Brown paper)	Sulphur
Ivory	Silk	(Gas carbon)	Gutta-percha
Rock-crystal	The hand	(Ebonite)	(Celluloid)
Glass	Wood	Indiarubber	—

The general order is taken from Ganot's " Physics," but the materials inserted in brackets are my addition. This is the normal series, but it must be understood that varying hardness and composition will cause changes in the order.

Experiments.

Commence with the pair *glass/silk,* and be careful to use throughout the same specimen of each :—

1. Having obtained the normal relation glass $+$/silk —, place the former in (a) a clear bunsen flame. We then find glass —/silk $+$. Render the glass normal (see above) and repeat the experience for (b) a smoky bunsen flame, (c) an alcohol flame, (d) a benzene flame. Since all the flames operate equally well, the effect cannot be attributed to chemical peculiarity, such as, for instance, a trace of sulphur in (a) or unoxidised carbon in (b). The effect increases up to a limit with time exposure in the flame.

2. After the glass has been in the flame there is a sticky feeling about it which may be due to moisture, but does not seem like it. The abnormal effect is greater when the stickiness is greater. But nothing is visible on the surface of the glass under a high power microscope.

3. In one instance the abnormal glass was put aside for 12 days. It remained abnormal at the end of the time.

4. Dip the abnormal surface in water and let the latter dry off. The abnormal state remains.

5. Instead of letting the water evaporate off the rod wipe it off with a cotton duster. If the duster be dry we have glass $+$/cotton—, but if damp we have glass —/cotton $+$. The latter effect will pass away in time, as friction continues, leaving the rod normal.

6. After abnormal glass has been excited for some time by silk we get glass $+$/silk —. Now remove the charge from the glass (*over*, not *in*, a flame) and again excite with silk ; we often

find glass —/silk +. This after effect is weak and is soon removed.

7. Prepare a small electric furnace. A test tube of vitreous silica is surrounded by a heating coil and the whole packed in slag wool. Now place a rod of normal glass in the furnace by the side of a platinum thermometer. Raise the temperature to 650 deg., which is just short of the melting point of the glass used. Remove the glass and allow it to cool quickly, or, leaving it in the furnace, shut off the current and let it cool slowly. In either case the glass becomes abnormal when cold. Next raise the glass to 720 deg. and melt it. The result is as before.

8. Raise the furnace to 800 deg. Place the normal glass rod in it and remove after two seconds. When cold the glass is abnormal to silk.

9. Ten seconds exposure of the glass rod in the furnace will discharge it whether it is normal and charged + or abnormal and charged —. But it always emerges abnormal.

10. Exposure of the glass in the blowpipe flame for one second renders it as abnormal as 20 seconds in the furnace. In the first case the surface is barely warm, in the second it is very hot.

11. Place normal or abnormal glass in a blowpipe flame and melt the glass and allow to cool quickly. When cool it is always abnormal.

12. Place normal glass in the cold furnace. Raise the temperature slowly to 700 deg. Then stop the current and let whole cool very slowly, thus annealing the glass. When cold the glass is abnormal. But the effect is so slight that we here have confirmation of the theory that the abnormal effect is due to strain.

13. Excite normal glass with indiarubber. It becomes +. Pass it through a flame and excite again. It is feebly —. Excite with silk it is strongly —. Thus glass, which normally is 5th in the series, comes when abnormal below indiarubber, which is 15th in the series.

The foregoing experiments show that (a) glass when heated has its surface so transformed that it descends in the series from its usual high place to one at least below indiarubber. (b) The effect is superficial, being found after fusion of the material as a whole and also after the glass is warmed slightly in a flame ; though, no doubt, in the latter case, the surface layers are subject to high temperature and great strain. (c) The effect does not pass away by immersion in water or if the

surface is breathed on, or (d) by contact with the air for a week or two.

Next proceed to test solids other than glass.

Vitreous Silica/Silk.

1. Pass a normal silica rod through the flame. It is now abnormal. It holds its state while it is rubbed with silk 15 times. If glass be treated in the same way it requires only five rubs to make it normal. Thus silica shows the effect better and retains it better than glass.

2. Raise silica to the highest attainable temperature, say 1,200 deg., in the blowpipe flame, or bring it to 900 deg. in the special furnace. Whether cooled slowly or quickly it is abnormal in each case when cool.

3. The after effect observed in glass (see 6) is more pronounced in silica. This after effect is, perhaps, due to some molecules on the surface acting normally and some abnormally at the same time. The explanation of the after effect would then be as follows :—

When the abnormal glass surface is rubbed sufficiently a majority of the surface particles become normal in action, but these would at first be in a transition state. They would be unstable, readily becoming abnormal. The + charge produced on the glass is removed by the free ions above the flame, but the slight heat experienced by the surface layers when over the flame may suffice to bring these unstable particles back to abnormality. This unstable effect would then be analogous to that sometimes found in a group of many neighbouring equispaced magnets (as in Sir J. A. Ewing's models) when influenced by a transitory field.

In all the following pairs the substance placed first becomes abnormal after passing through the flame :—

Ivory/silk.
Wood/indiarubber.
Copper/indiarubber.
Copper/brown paper.
Steel/indiarubber.
Lead/indiarubber.
Brown paper/sealing wax.
Gas carbon/indiarubber.
Slate/indiarubber.
Vulcanised fibre/silk.
Ebonite/indiarubber.

Q

Of some 18 pairs tested only one fails to give the abnormal effect. This is indiarubber sealing wax. But rubber is the softest substance used, and the general law is that hard substances show the effect well and soft ones ill. Probably the effect occurs in rubber, but can only be observed by more delicate means.

All substances when abnormal stand below indiarubber in the electrification series.

In making these observations some general precautions are to be observed : (a) Many of the substances conduct more or less well. These must be mounted for a short length in a tube of glass or silica to avoid conduction and induction of the hand. (b) When the abnormal effect is slight, care is required to observe whether a downward movement of the gold leaf is due to charge on the rubbed body or is merely an induction effect by it. When in doubt the leaf is used discharged. (c) In many cases the charges are only producible by *lightly* brushing one solid by the other. Thus the — charge, when abnormal metal is excited by rubber, can only be produced in this way.

Review.

As to the cause of the abnormal effect. It cannot be attributed to any organic substance deposited by a flame, since the effect is producible without flame. It cannot be due to the removal of a layer of condensed water vapour, since after immersion in water the effect remains on the surface. Nor is it due to the deposition by flame of water on the cold surface, since flame is not necessary. Again, it seems unlikely to be caused by removal by heat of the air film, since it remains on the surface 12 days or more after the time of production, the solid being in the air during the whole time. If, then, the effect is not due to a layer *on* the surface of the solid, we must regard the surface layers of the solid as the seat of the action. Whether in producing the effect the solid be surrounded by air in a furnace or by flame, there must be sudden great agitation and subsequent strain imposed on the surface layers of molecules. Under these circumstances the surface readily produces — when excited. The process of rubbing, especially in the case of soft bodies, would relieve the state of strain and the surface would be restored to normal. Any closer conjecture would seem out of place at present.

One or two tests were applied as to the surface action. (a) The orientation of surface atoms in the magnetisation of steel

might have some relation to the strain effect we are considering. A strong magnetic field was applied to a steel bar both when normal and abnormal. The bar was also subject to repeated reversals of field. But no change seemed to occur in any case. (b) It was thought that sudden lowering (like sudden raising) of temperature might have an effect on the surface layers, causing the body to rise or fall in the electrification series. The lowest available temperature was that of freshly-prepared liquid air. This seemed to have no influence on silica, glass, brass or sealing wax, whether these were normal or abnormal. But this lowering of 200 deg. is small cf with the rise, 1,600 deg. in the case of a bunsen flame, so the negative result here may merely indicate that the shock is not severe enough. One objection to cooling the solid is that water vapour soon condenses from the air on the surface, and this freezes and thus alters the conditions for rubbing.

There are few recent researches directly bearing on the point raised in this Paper. Two may be mentioned :—

W. Jamieson (" Nature," 83, p. 189, April 14, 1910) found that the convex side of a bent strip of celluloid or mica is $+$ to the hand, whereas the concave side is —. I have not succeeded in reproducing this effect. If it exists, we see that the surface under tension gives $+$, whereas that under compression gives —.

Sir J. J. Thomson (Camb. Phil. Soc. " Proc.," 14, p. 105, March 6, 1907) found that salts heated to 300°C. give off charges. Some salts produce $+$, others —. The sign of the charge remaining in the salt is always the same as that produced by friction. The salts appear to be covered by a double layer of electrification, and it is suggested that a double layer occurs on the surface of all solids. When electrification by friction takes place one or both of these layers are rubbed away.

The subject of frictional electrification has so far been handled qualitatively only ; hence our present ignorance on the subject. But there should be a fruitful field of research in treating it quantitatively. Something useful might be discovered by frictional work in vacuo.

A glass rod was raised in temperature slowly to dull redness and slowly cooled (say in two hours) to ordinary temperature. It was then found to be abnormal. The annealing here attempted may be imperfect, but if the best possible annealing

leaves the substance " abnormal " we shall have to reverse
the terms normal and abnormal as used above, since the
annealed state must surely be the normal one and the state
attained after rubbing would be abnormal or strained.

With a view to removing the surface layers, hydrofluoric
acid was applied to an abnormal glass rod. After washing in
water and wiping, the glass surface was slightly abnormal.
From this it might seem that the layers of glass under the sur-
face are always abnormal. This we should expect from the
experiment immediately preceding. But the result appears
nugatory for two reasons : (1) A foreign substance which acts
chemically on the surface upsets all the conditions ; (2) the
acid etches and roughens the glass surface, and it is well known
that " rough " glass is in general negative to silk.

As a further test on the after effect mentioned above, a glass
rod was made abnormal, then thoroughly normal —*i.e.*, it was
not possible to get any after effect in the usual way. It was
then put aside for several days. It was then found to be
normal still. Hence we see that the normal and abnormal
states are both stable if well established, but that there is an
uncertain intermediate state.

As one result of these experiments we obtain a rule for dis-
charging charged surfaces :—

Place the charged surface some distance, say 15 cm., above
or at the side of a flame, *never in* the flame, and keep it there
for a few seconds only at a time. If the body be kept in the
flame for a second or two, or be kept over the flame for a
minute or two, it will be discharged, but it will also be abnormal.

ABSTRACT.

I. The Paper deals with the anomalous electrical behaviour of
various substances when subjected to heat. For example, a glass rod
rubbed with silk is normally left positively electrified, but if the rod
be passed through a bunsen flame, or heated in an electric furnace,
and then allowed to cool, it will be found on again rubbing with the silk
that the glass becomes negatively electrified. The reversal in sign, here
called abnormal, can be produced (*a*) in a clear or smoky bunsen flame,
(*b*) in a blowpipe flame, (*c*) in a benzene flame or an alcohol flame, (*d*)
over any flame if enough time is allowed, (*e*) in an electric furnace, where
heat reaches the surface affected by radiation through air.

II. Discharge of any charge + or — which may be on the surface
in every one of the above cases precedes the production of the abnormal
state.

III. The abnormal state can be removed from the surface by con-
tinued rubbing with silk, cotton, &c., or, better, by rubbing the surface

with the hand. It is not removed by melting (in the case of glass) or by annealing, or by the action of water or by lapse of time.

IV. It is a surface effect only.

V. An unstable intermediate condition is found after the surface has been rubbed for some time. In this condition the surface may act normally when rubbed in one way, abnormally when rubbed otherwise. This state does not last.

VI. All solids which will stand the action of a flame for a second or two appear to act, as does glass, in the above ways. The action is seen best in vitreous silica and less well in wool, metals, slate, paper, ebonite, &c. In all cases the substance rubbed is, when normal, above india-rubber the ordinary frictional list, but below it when abnormal.

DISCUSSION.

Prof. RICHARDSON said he had thought the effect might have something to do with the emission of ions when the rod was heated, but the details did not fit in with this. It probably resulted from some mechanical or chemical change in the surface molecules.

Mr. F. E. SMITH said it was a familiar fact to teachers of physics that glass would sometimes behave in unexpected ways in frictional experiments. It had always been his habit before rubbing the glass to pass it through a flame, but the glass was usually positive after rubbing with silk which had been treated with amalgam. Possibly the amalgam was responsible for the absence of any abnormal effect. It seemed significant that the abnormal effect could be destroyed by passing through the hand. The moisture of the hand was alkaline, as was also the moisture usually found condensed on substances, and which the flame would naturally remove. He suggested pouring mercury through funnels of different materials as a means of exciting electrification. This would eliminate uncertain effects in one member of the pairs and might simplify the investigations.

Dr. C. CHREE asked if the whole of the surface was found to be in one state either normal or abnormal at the same time.

Prof. HOWE asked whether, if a flame were played against one side of a glass plate, the plate would act normally on one side and abnormally on the other.

Prof. S. W. J. SMITH thought that devitrification of the glass or silica on heating might have something to do with the phenomenon, though the effect of an action of this kind would not, of course, be destroyed by the simple expedients found to be effective by the author.

Mr. G. L. ADDENBROKE mentioned some experiments of his, in which the effect of moisture on leakage of condensers had to be investigated. Usually the surface of glass was comparatively conducting, and was made much more so by warming to about 30°C. If, however, it were dried by heating, then, so long as the glass was kept slightly warmer than the air—1°C. was enough—condensation seemed to be arrested and the glass remained dry. The glass should invariably be washed with distilled water, or the condition of its surface was quite uncertain.

Dr. SHAW, in reply, said that Mr. Smith's suggestion of running mercury over the surface under test had not been tried by him. It should prove easy and useful. As to Mr. Smith's suggestion that the alkaline surface of glass and the acidic surface of silica might influence the effect, he would group that with Dr. Smith's idea of devitrification as possibly having some force in the cases of glass and silica, but not for the vast variety of other solids for which the effect has been found. Mr. Addenbrooke's remark as to thoroughly washing the glass would also seem to bear on some only of the substances used; but one would expect that in any case the normal state of the surface

as to covering layers would return in a week, which is not so. Dr. Chree had considered the possibility that the surface might be normal or abnormal in patches, but there was no evidence at hand. If the effect was attributable to strain it might be possible to get one effect on the convex side and the contrary on the concave side of a bent glass plate. Dr. Shaw had tried this effect, but had failed to observe any difference, though he was aware tension and compression were supposed to produce contrary effects. The subject raised in this Paper raised great theoretical possibilities, and should next be taken up quantitatively and the surfaces treated in vacuo.

XVIII. *Electromagnetic Inertia and Atomic Weight.* By J. W.
NICHOLSON, *M.A., D.Sc., Professor of Mathematics in the
University of London.*

IT is generally believed that the experiments of Kaufmann
and Bucherer have finally demonstrated that the mass of a
swift β particle, and, therefore, of any electron, is purely of
electromagnetic origin, and in accordance with the formula of
Lorentz. This mass is accordingly, on the supposition that a
slowly-moving electron is spherical, of the form ae^2/ac^2, where
a is a numerical factor, quoted usually as $\frac{2}{3}$, and e and a are
the charge and radius of the electron, c being the velocity of
light in free aether. Modern theories of the nature of positive
electricity in an atom—theories to which we are led inevitably
by many converging lines of experiment—demand also a
discrete structure for positive electricity. It must exist in
portions equal to e or multiples of e, and of a size not greater
than that of the electron, but of much greater mass. The
similarity of these portions to electrons suggests that their
mass is also purely electromagnetic, and if so, it should be sub-
ject to the same formula, which, as dependent on velocity,
unfortunately cannot be tested, by virtue of the difficulty of
giving a sufficiently high velocity to such comparatively heavy
bodies as α-particles.

Rutherford's model of the hydrogen atom contains only a
single electron and a single *positive electron.* But a hydrogen
atom has the mass of 1,835 electrons, according to the best
estimates, and therefore the positive electron is equivalent on
this view, in spite of the identity of charge, to 1,834 electrons in
mass. Since the radius of the electron is of order 10^{-13} cm.,
that of the positive electron is of order $\frac{1}{2}10^{-16}$ cm., and
this greater concentration is responsible for the increase of
mass.

Most of the properties of the hydrogen atom, and, in fact,
all that are explicable on this basis, would, however, remain
unchanged if the nucleus of the atom were not a positive elec-
tron, but an aggregate of nuclei and electrons whose total
charge amounts to $+e$. There are many indications that this
is a more correct view, and some of them may be reviewed
briefly. In the first place, there is the modern chemical

theory of isotopic elements, introduced by Fajans, Soddy,[*]
and Fleck, apparently supported by the system which it intro-
duces into the arrangement of the products of radioactive
elements in the periodic table, and by the probable existence of
two varieties of neon with nearly the same atomic weight, dis-
covered originally by Sir J. J. Thomson, and further investi-
gated by Aston.[†]   The position of an element in the periodic
table, in this theory, depends not on its atomic weight, but
on its *atomic number*, which, if not identical with, is certainly
closely related to the charge on the atomic nucleus.   Elements
whose nuclei contain quite different aggregates of positive and
negative electrons, and, therefore, different masses, are for all
practical purposes identical if the aggregate in the two cases
have the same resultant positive charge   That these atomic
numbers have a real significance is proved conclusively by the
work of Moseley.[‡]   If this theory be accepted, we may inquire
at what point in the periodic table this nuclear structure may
be supposed to start.   The existence of neon and metaneon,
if they are actually isotopes, as seems necessary, shows that it
has commenced long before the elements recognised as radio-
active are reached.   A favourite supposition is that it com-
mences after helium, for all known α-particles appear to be
nuclei of the helium atom.   Such particles might, therefore,
be the simple units which go to the making of a compound
nucleus.   But the existence of hydrogen, a simpler element,
precludes the possibility that the helium nucleus is the simplest
which can enter into such structures, and although suggestions
have not been lacking that the nuclei of all elements are com-
posed of hydrogen and helium nuclei, no experimental evi-
dence of the existence of these hydrogen nuclei has been
derived hitherto from the phenomena of radio-activity.   In
any case, helium nuclei cannot be the simplest forms of positive
electricity, and we are tempted to suppose that complexity of
structure extends throughout the nuclei of the terrestrial
elements, and probably even to hydrogen.   For the experi-
ments of MM. Buisson and Fabry[§] have shown that the
secondary spectrum of hydrogen is largely due to atoms, and
even by Bohr's method [||] we cannot explain the existence of

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[*] " The Radio-Elements and the Periodic Law," Longmans, Green & Co.,
1914.
[†] British Assoc. Report, Birmingham, 1913.
[‡] " Phil. Mag.," 1913, VI., 26, p. 1024, Royal Society Discussion, 1914.
[§] " Journal de Physique," June, 1912.
[||] " Phil. Mag.," July and Nov., 1913.

so rich a spectrum without a more complicated model atom, even if the complication does not extend beyond the confines of the nucleus.

The charge on the nucleus of an atom is roughly, but in the case of the lighter elements only very roughly, proportional to the atomic weight. If these elementary nuclei were simple charges and not structures, some law of a simple character should connect these two quantities. It is precisely in the simplest elements that no such law can be found, even approximately. It seems necessary to believe that their nuclei are aggregates of charges, for which each individual member follows a simple law of mass which is hidden by the varying numbers of them in the aggregates. In heavier elements with very complex aggregates, the mass becomes proportional very closely to the number of charges in the aggregate, and follows another simple law which hides the first.

It is now almost certain that this conception represents the facts. For Nicholson* has shown that the spectra of unknown origin in the nebulæ and solar corona can be interpreted with great accuracy by the vibrations of atoms of the Saturnian type containing 3, 4, 5, and 6 electrons rotating round simple or non-structural nuclei of charges $3e$, $4e$, $5e$ and $6e$. The masses of these nuclei are at the same time determined from the spectra, in terms of the mass of the electron, and the ensuing atomic weights of the substances are, as expected, not those of known elements. For example, the nucleus $6e$ leads to an atomic weight 2·9, and MM. Bourget, Buisson and Fabry,† by applying their interference method to some of its lines in the nebular spectrum, have verified this value, showing at the same time, by accurate interference measurements of the lines, that they are found in the spectrum of no known element, and must be accredited to a new one. The dissimilarity between the representations of these spectra by formulæ and the corresponding representations for the spectra of terrestrial elements points to an extreme degree of simplicity in these elements, although some are heavier than hydrogen. But the most significant fact is that their calculated nuclear masses follow a simple law very accurately. They are proportional to n^2, where ne is the charge on the nucleus, and this is a natural law to expect—if they are really single portions of positive

* Monthly notices of Royal Astron. Soc., 1913-1915.
† " Journal de Physique," May, 1914.

electricity, concentrated into spheres of the same size—as the ordinary formula for mass indicates.

Now, according to this theory, the mass of a *positive electron* of charge e is only $2 \cdot 9 \ 6^2$, or $0 \cdot 081$ of that of a hydrogen nucleus. Its radius is therefore $1/(1.835 \times 0.081)$, or about $1/150$ of that of an electron—or 13 times Rutherford's estimate. To obtain the mass of any *simple* positive charge ne, we use the expression $0 \cdot 081 n^2$, which rapidly becomes large as n increases. If $n=7$, we already have an atom as heavy as that of helium, and if $n=14$, as heavy as that of oxygen. But the atom of hydrogen must have a complex nucleus, and even its atomic weight is not that of a single particle of positive electricity.

Recent work on radio-activity, and on the effects of atoms in scattering α-and β-particles has also led to the conclusion that α-and β-particles come from the nucleus of the atom, which is accordingly complex. The older theory that β-particles came from an inner ring of electrons in orbital motion has been shown to be untenable on the theoretical side,* for such rings cannot exist in this type of atom, whether the atom is subject to ordinary dynamics, or the type of analysis used by Bohr † in his theory of the hydrogen spectrum. We must conclude, therefore, that, on all counts, the nuclei of the elementary atoms are structures of positive and negative electrons packed tightly together.

The tightness of this packing is instanced by the fact that one deduction made by Rutherford from his experiments, and from the analysis of Darwin, is that the law of action from the nucleus is still that of the inverse square at distances comparable with 10^{-12} cm., or with 10 times the radius of an electron. An assemblage of particles, each individually exerting this law of force, would give a different law on the whole unless their distances apart were small compared even with 10^{-12} cm. The distances apart are, therefore, of the order of the radius of an electron, and in the case of positive electrons, probably still smaller. In these circumstances, an important question arises, for we know from general considerations alone that the joint mass of two charges close together cannot be quite the sum of the individual masses, if all mass is really electromagnetic. The interaction of their fields must be taken into account ; some mode of calculation of the mass of a compound nucleus is required. In the next section this problem is treated

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* Nicholson, " Phil. Mag.," April and July, 1914.
† *Loc. cit.*

mathematically. The mass is derived in the usual way by setting the charges into motion with a small velocity, and working out the magnetic energy of their field. For simplicity, they are made to move in their common line with equal velocities.

Mass-formula for Two Charges.

If a small charge e moves along the axis of z in a co-ordinate system, the components of magnetic force (α, β, γ) which it produces at a point in external space are

$$\alpha = \frac{eu}{c}\frac{\partial}{\partial y}\left(\frac{1}{r}\right), \quad \beta = -\frac{eu}{c}\frac{\partial}{\partial x}\left(\frac{1}{r}\right), \quad \gamma = 0, \quad . \quad . \quad (1)$$

where u is its velocity. The charge is at the origin at the instant considered, and (α, β, γ) is the magnetic force at a point (x, y, z) at distance r.

If the charge is at the point (o, o, c) on the z axis, the magnetic forces at an external point (x, y, z) are

$$\alpha = -\frac{euy}{cr^3}, \quad \beta = \frac{eux}{cr^3}, \quad \gamma = 0,$$

where $r^2 = x^2 + y^2 + (z-c)^2$.

If there are two such charges on the axis of z, each moving along it with velocity u,

$$\alpha = -\frac{uy}{c}\left(\frac{e_1}{r_1^3} + \frac{e_2}{r_2^3}\right), \quad \beta = \frac{ux}{c}\left(\frac{e_1}{r_1^3} + \frac{e_2}{r_2^3}\right), \quad \gamma = 0, \quad . \quad . \quad (2)$$

where r_1 and r_2 are the distances from (x, y, z) to the two charges. The resultant magnetic force is H, where

$$H^2 = \frac{u^2}{c^2}\left(\frac{e_1}{r_1^3} + \frac{e_2}{r_2^3}\right)^2(x^2 + y^2), \quad . \quad . \quad . \quad . \quad (3)$$

and the magnetic energy is

$$\iiint \frac{H^2}{8\pi}dxdydz = \frac{u^2}{8\pi c^2}\iiint\left(\frac{e_1}{r_1^3} + \frac{e_2}{r_2^3}\right)^2(x^2 + y^2)dxdydz, \quad (4)$$

the integrations extending throughout the space external to both charges. This is to be identified in the usual manner with $\frac{1}{2}mu^2$, where m is the *mass* of the pair of charges. The mass of the first charge moving alone is

$$\frac{e_1^2}{4\pi c^2}\iiint \frac{x^2 + y^2}{r_1^6}dxdydz = \frac{2}{3}\frac{e_1^2}{a_1 c^2}, \quad . \quad . \quad . \quad (5)$$

if a_1 is its radius, by a well-known calculation. Similarly

$$\frac{e_2^2}{4\pi c^2} \iiint \frac{x^2+y^2}{r_2^6} dx dy dz = \frac{2}{3} \frac{e_2^2}{a_2 c^2}. \quad . \quad . \quad . \quad (6)$$

if a_2 is the radius of the second charge. But the mass of the pair is

$$\frac{2}{3c^2}\left(\frac{e_1^2}{a_1} + \frac{e_2^2}{a_2}\right) + \frac{e_1 e_2}{2\pi c^2} \iiint \frac{x^2+y^2}{r_1^3 r_2^3} dx dy dz. \quad . \quad . \quad (7)$$

The last integral is not zero, and it represents the mutual effect of their fields. Its value must now be calculated, and it will be referred to as the " mutual mass " of the two charges.

Consider now the order of magnitude of the integral when taken throughout one of the charges, the distance between their centres being λ, as in the figure. Then r_1 lies between the
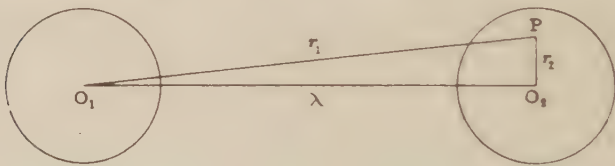
values $\lambda \pm a_2$, and the integral between the values

$$\frac{e_1 e_2}{2\pi c^2} \frac{1}{(\lambda \pm a_2)^3} \iiint \frac{(x^2+y^2)}{r_2^3} dx dy dz,$$

which becomes when transformed to spherical polar co-ordinates of origin 0_2,

$$\frac{e_1 e_2}{2\pi c^2} \frac{1}{(\lambda \pm a_2)^3} \int_{r=0}^{a_2} \int_{\theta=0}^{\pi} \int_{\phi=0}^{2\pi} r_2 \sin^3\theta \, d\phi \, d\theta \, dr_2,$$

or, on reduction,

$$\frac{2}{3} \frac{e_1 e_2}{c^2} \frac{a_2^2}{(\lambda \pm a_2)^3}. \quad . \quad . \quad . \quad . \quad . \quad (8)$$

For two equal charges actually in contact, this contribution would be as important as the mass of each individual charge. But its relative importance is determined by the ratio $(a_2/\lambda)^3$, and if λ is even so small as $5a_2$, a formula which neglects it would be correct within about 1 per cent.

In a complex nucleus, we may suppose, for reasons to be given later, that the distances between the various α-particles

must be nearly of this order, and we may then regard this integral as negligible. In other words, instead of taking the integral in (7) over all space external to both charges, we may take it over *all* space, either external or internal. The work of calculation is thereby greatly simplified. But if the charges are closer together this procedure cannot be justified.

Let, then, λ be the distance between the charges, and let $r_1 = r$. If we take 0_1 in the figure as the origin of polar co-ordinates (r, θ, φ) of any point in space,

$$r_2^2 = r^2 + \lambda^2 - 2r\lambda \cos \theta,$$

$$dxdydz = r^2 \sin \theta d\varphi d\theta dr, \quad x^2 + y^2 = r^2 \sin^2\theta,$$

and the mutual mass is

$$m_{12} = \frac{e_1 e_2}{2\pi c^2} \int_{r=0}^{\infty} \int_{\theta=0}^{\pi} \int_{\varphi=0}^{2\pi} \frac{r \sin^3\theta d\varphi d\theta dr,}{(r^2 + \lambda^2 - 2r\lambda \cos \theta)^{\frac{3}{2}}}$$

$$= \frac{e_1 e_2}{c^2} \int_0^{\infty} \int_0^{\pi} \frac{r \sin^3 \theta dr d\theta.}{(r^2 + \lambda^2 - 2r\lambda \cos \theta)^{\frac{3}{2}}}$$

If $\cos \theta = \mu$, $r = \lambda v$,

$$m_{12} = \frac{e_1 e_2}{\lambda c^2} \int_0^{x} v dv \int_{-1}^{1} \frac{(1-\mu^2)d\mu}{(1 - 2\mu v + v^2)^{\frac{3}{2}}}. \quad \quad \cdots \quad (9)$$

The double integral is entirely numerical, so that the mutual mass is of the form $a e_1 e_2 \lambda$, where a is a pure number, and λ is the distance between the centres of the charges.

Now,

$$\int_0^{x} \frac{v dv}{(1 - 2\mu v + v^2)^{\frac{3}{2}}} = \int_0^{\mu} \frac{v dv}{[(\mu-v)^2 + (1-\mu^2)]^{\frac{3}{2}}} + \int_{\mu}^{x} \frac{v dv}{[(v-\mu)^2 + (1-\mu^2)]^{\frac{3}{2}}}$$

In the first of these integrals, write

$$\mu - v = (1 - \mu^2)^{\frac{1}{2}} \tan \varphi,$$

where φ ranges from 0 to $\sin^{-1} \mu$, and in the second,

$$v - \mu = (1 - \mu^2)^{\frac{1}{2}} \tan \psi,$$

where ψ ranges from 0 to $\pi/2$, and we obtain

$$\int_0^{x} \frac{v dv}{(1 - 2\mu v + v^2)^{\frac{3}{2}}} = \frac{1}{1 - \mu^2} \int_0^{\sin^{-1} \mu} (\mu \cos \varphi - \sqrt{1 - \mu^2} \sin \varphi) d\varphi$$

$$+ \frac{1}{1 - \mu^2} \int_0^{\pi/2} (\mu \cos \psi + \sqrt{1 - \mu^2} \sin \psi) d\psi.$$

These integrals are easily evaluated, becoming respectively $\mu+\sqrt{1-\mu^2}$ and $1-\sqrt{1-\mu^2}$, and accordingly

$$\int_0^\infty \frac{v dv}{(1-2\mu v+v^2)^{\frac{3}{2}}} = \frac{1}{1-\mu^2}\{\mu-\sqrt{1-\mu^2}+1-\sqrt{1-\mu^2}\} = \frac{1}{1-\mu} \quad (10)$$

Thus, from (9)

$$m_{12} = \frac{e_1 e_2}{\lambda c^2}\int_{-1}^1 (1-\mu^2)\frac{d\mu}{1-\mu} = \frac{e_1 e_2}{\lambda c^2}\int_{-1}^1 (\mu+1)d\mu = \frac{2e_1 e_2}{\lambda c^2}. \quad (11)$$

The mass of two charges e_1, e_2 of radii a_1, a_2 at distance λ apart is therefore

$$\frac{2}{3c^2}\left(\frac{e_1^2}{a_1}+\frac{e_2^2}{a_2}+\frac{3e_1 e_2}{\lambda}\right), \quad \ldots \ldots \quad (12)$$

and the proximity of electrical charges destroys the conservation of mass. The mass is increased if two charges of like sign are brought closer, but decreases if they are of opposite sign. When they are *very* close together, this formula ceases to be applicable, but it can be used so long as λ is not greater than about five of their radii.

We can at once extend the result to any number of charges in proximity to one another. If e_r and e_s are two of them at distance λ_{rs} apart, the total mass is

$$\frac{2}{3c^2}\left\{\Sigma\frac{e_r^2}{a_r}+3\Sigma\Sigma\frac{e_r e_s}{\lambda_{rs}}\right\}.$$

Consider, for example, an atom whose nucleus consists of a simple positive charge of amount m_1, and a positive charge, also simple, of amount $2e$—the form assumed by Rutherford for the α-particles—together also with two electrons. Such an elementary atom would have perfect isotopy with a simpler element whose nucleus consisted *only* of a positive charge, m_1. It could change into this simpler element by the emission of one α-particle and two β-particles. Such cases, in which an element returns, after two or perhaps three operations, to the same place in the periodic table, are of frequent occurrence in radioactivity, according to the theory of isotopes. The atomic mass of the element before these emissions is made up of (1), the mass m of the external electrons, n in number, rendering the atom neutral, (2) the mass m_1 of the nucleus m_1, (3) the masses m_3 and $2m_4$ of the α-particle and the two nuclear electrons taken individually, and (4) the *mutual* masses of (3) and (2). After the emissions there are left only (2) and (1).

The radio-active change has, therefore, decreased the mass of the atom by $m_3 + 2m_4$, together with the mutual masses. We may neglect the individual masses of a small number of electrons like 2, as is ordinarily done in calculations. The mutual masses of the combination become, if λ is the mean distance between the centres of the components,

$$\frac{2e^2}{\lambda c^2}\{2n - (n+n) - (2+2) + 1\} = -\frac{6e^2}{\lambda c^2},$$

independent of n. The first term is the mutual mass of nucleus and α-particle, the second of nucleus and each electron, the third of α-particle and electrons, and the fourth of the two electrons.

The loss of mass by the emissions is therefore $m_3 - \dfrac{6e^2}{\lambda c^2}$, and not m_3, the mass of a helium atom, as ordinarily assumed. The magnitude of the correction is $9ma\,\lambda$, where m and a are the mass and radius of an electron, and this is only comparable with the mass of a single electron.

Atomic weight calculations for radioactive products are not therefore seriously affected if the element ultimately ends as an isotope of its first form. But if it changes its position in the table the case is different. For example, the total passage from radium to lead involves a change of 10 in the atomic number, so that, on the whole, an amount $20e$ of positive electricity has left the nucleus in the form of 5 α-particles. In the radium atom the total mass is that of the lead nucleus and its attendant external electrons, of the five individual α-particles, and the mutual masses of the members of this group. Neglecting a few electrons, and also mutual masses involving the distant external electrons, we find that the absolute mass of a radium atom should exceed that of a lead atom by

$$5A + \frac{5(5-1)}{2}\cdot\frac{2}{\lambda c^2}(2e)^2 + 10ne^2 \cdot \frac{2}{\lambda c^2},$$

where A is the mass of a helium atom, n is the atomic number of lead—about 103—and λ is the average distance apart of the components in a radium nucleus. The factor $5(5-1)/2$ expresses the number of ways of grouping the α-particles in pairs and the last term is the mutual mass of the lead nucleus and the α-particles. The expression becomes

$$5A + 2040\,\frac{e^2}{\lambda c^2}.$$

If a is the radius of an electron, the mass of a hydrogen atom is $\dfrac{2}{3}\dfrac{e^2}{ac^2} \times 1835$. The decrease of atomic weight in passing from radium to lead would, therefore, be, since 3·994 is the atomic weight of helium,

$$5(3\cdot994)+\frac{2040\times3}{2\times1835}\frac{a}{\lambda}=19\cdot97+1\cdot65\frac{a}{\lambda}.$$

The atomic weight of lead derived from radium should, therefore, be, if we use Hönigschmid's determination of the atomic weight of radium (225·95).

$$205\cdot98-1\cdot65\frac{a}{\lambda}.$$

where λ is a rough mean of the distances between a-particles in the atom, and a is the radius of the electron.

We can make a similar calculation for lead derived from thorium, taking $n=116$, and allowing for 6 a-particles instead of 5. The result is, if λ is the same as before, and if the atomic weight of thorium is 232·4,

$$208\cdot4-2\cdot40\frac{a}{\lambda}.$$

The accepted value for the atomic weight of ordinary lead is 207·10, and it is regarded as a mixture of leads from both sources. Actinium as a source is relatively unimportant. If we suppose the mixture to be in equal proportions, then on taking the mean,

$$207\cdot10=207\cdot19-2\cdot02\frac{a}{\lambda},$$

or,

$$\frac{\lambda}{a}=2\cdot5,$$

so that the average distance between the components of a nucleus is about 2½ times the radius of an electron. As we are uncertain about the proportions, however, this calculation is unconvincing.

But Soddy has just given[*] a careful determination of the atomic weight of thorite lead, of which a large quantity has been isolated. The value is 207·64, and we may suppose that

[*] Vide, e.g., "Nature," Feb. 4, 1915.

lead from radium is here absent. Applying the formula, therefore,

$$208\cdot40 - 2\cdot40\frac{a}{\lambda} = 207\cdot64,$$

or,
$$\frac{\lambda}{a} = 3,$$

almost exactly. This value therefore compels the average distance between the a-particles to be of the same order as the radius of an electron. Under this condition, the formula we have developed ceases to give the mutual mass of an electron and a positive charge. But it would still remain valid for positive charges, on account of their smaller size, and since in the passage from thorium to lead, positive charges are mainly in question, some reliance can be placed on the result. We have reached the conclusion, therefore, that if the atomic weight of thorite lead has the value found by Soddy, and if the higher value is not due to an unsuspected impurity, *the a-particles in a thorium atom have a mean distance apart comparable with the radius of an electron.* The importance of this conclusion in connection with atomic structure is evident, and on the supposition that all mass is electromagnetic, it is impossible to evade it.

In particular, it is a valuable indication that the nucleus of the atom is actually of very small dimensions and perhaps not very much greater than the radius of an electron. But it must be decidedly larger than Rutherford's estimate.

At the same time, these results appear to preclude any connection between the chains of development of uranium and thorium. For the atomic weight of lead from radium should be about 205·43, different from that of thorite lead by 2·21. If the chains are connected, they can only differ in their final products, the two leads, by about a multiple of 4 in atomic weight, unless we admit the existence of a new type of a-particle. The effect of mutual mass is not significant enough, *in the case of isotopes,* as we have seen, to alter this number 4. Thus, if lead is the ultimate product of radium, *all* the a-particles have been observed experimentally. On the supposition that ordinary lead has its origin in thorium and in radium, we can work out, from its atomic weight, the proportions of the two components. Thus, if there are x parts of thorium lead to one of radium lead,

$$207\cdot64x + 205\cdot43 = (x+1)(207\cdot10),$$

or $x = 3$.

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Thus, $\frac{3}{?}$ of the lead should arise from thorium. This is a point which could be tested on the basis of the present terrestrial distribution of thorium and uranium.

Many other questions are suggested by the calculation of the mutual mass of two charges. If, for example, charges really existed in the atom, closer to the nucleus than the outer ring of radius 10^{-8} cm., the mutual mass would make an important difference in their equations of motion, and would show its effect on the numerical constants of their spectra. Such an effect must be brought into any theory which involves charges moving in a small radius. But at a radius 10^{-8}, the effect is not appreciable with the order of accuracy which can be attained.

Apart from the deduction of the formula, which is the main object of the Paper, and the emphasis which must be laid on this question of mutual mass, almost completely overlooked hitherto in all theoretical discussion of the data of radio-activity, this Paper will have served its purpose if it succeeds in drawing attention to the great contributions to our knowledge of atomic structure, and more particularly nuclear structure, which can be given by some simple experiments, such as the precise determination of the atomic weights of lead from different sources, and by as many methods as possible. The actual conclusions in the last section are admittedly dependent on controversial points, for they involve the not generally accepted doctrine that the atomic weights of thorite lead and ordinary lead are different. Again, they involve the conclusion of Rutherford and Darwin that the law of force even at a distance of 10^{-12} cm. from the centre of a nucleus is still that of the inverse square. This conclusion, like the calculations in the Paper, would be overthrown if it could be proved that appreciable magnetic forces had their seat in the nucleus of an atom, as one school of physicists maintains. But the present calculations are the logical outcome of the suppositions just mentioned. Sir Ernest Rutherford, in the discussion on the structure of the atom at the Australian meeting of the British Association, proposed to leave nuclear structure to the next generation. It seems, on the other hand, to be vital to any further progress in our knowledge of atoms. The results already attained in the Paper are strongly in accord with the nucleus theory, since " mutual mass " is, on this theory, apparently capable of explaining the discrepancies which are observed in atomic weights.

ABSTRACT.

The Paper contains a mathematical deduction of a simple formula for the combined mass of two electrical charges when in proximity to each other. This mass is not the sum of their individual masses when far apart, if it be supposed that all mass of positive electricity, like that of electrons, is of electromagnetic origin. Applications are made of the formula to questions of atomic constitution and of radio-activity. A discussion is given of the evidence leading to the conclusion that the nuclei or cores of positive electricity in atoms are complex structures of electrons and even smaller positive nuclei. On this basis, emission of an a-particle by an atom does not decrease its atomic mass by 4, a correction being necessary for the " mutual mass " of the a-particle and the rest of the core. Estimates of the magnitude of this correction, in the case of radium and thorium passing into lead by the emission of particles, are given. From the value given by Soddy for the atomic weight of therite lead we can deduce the average distance apart of the components in a radium nucleus. It is of the same order as the radius of an electron. Suggestions of further interesting applications of the precise formula for mutual mass are also contained in the Paper.

DISCUSSION.

Dr. H. S. ALLEN said that, in the model suggested by Sir E. Rutherford, the atom consists of a concentrated positive charge of extremely minute size surrounded by electrons revolving at different distances from the nucleus. In earlier Papers Prof. Nicholson has shown that this is mathematically impossible, and that all the external electrons must either form a single ring or rings in parallel planes. The Rutherford atom is then reduced to such extreme simplicity that it becomes incapable of explaining the complex facts of physics and chemistry. It cannot, for example, give an explanation of the complicated series of lines actually observed in the spectra of elements other than hydrogen, or of the secondary spectrum of hydrogen itself. Again, such an atom should have a magnetic moment simply proportional to the number of electrons in the ring, for the nucleus is too small to produce by rotation, or otherwise, any appreciable magnetic moment. The results of the present Paper appear to prove that the nucleus of an ordinary atom cannot have the small dimensions previously assigned to it. It seems certain that the nucleus cannot be so minute if we assume that there exist in it a and β particles as such. The only reason for assigning an extremely small diameter to the nucleus of a heavy atom is to account for the wide-angle scattering of a particles, but in the discussion of this question no attention was paid to the possible action of magnetic forces on the moving particle. If these exist the trajectories become complicated, but it is probable that the scattering could then be accounted for without necessitating such a near approach to the centre of the atom. We are thus led to the view that the central portion of the atom may contain a and β particles in orbital motion which would set up a magnetic field. But as the velocities must presumably be less than that of light, the radius of such a *magnetic core* must be considerably greater than that of the simple nucleus of Rutherford.

XIX. *The Estimation of High Temperatures by the Method of Colour Identity.* By CLIFFORD C. PATERSON *and* B. P. DUDDING, *A.R.C.Sc.* (*From the National Physical Laboratory.*)

Synopsis.

1. Preliminary experiments are described on the method of " colour identity " adapted to the estimation of the temperature of incandescent substances such as metal or carbon radiating in the open ; by this method the " true " temperature of certain bodies as distinct from their " black body " temperatures can be arrived at with a very fair degree of accuracy.

2. By the colour identity method the total luminous radiation (white light) from a black body is made identical in colour with that from the incandescent metal under examination by adjusting the temperature of the black body until there is colour identity in the field of a Lummer-Brodhun photometer.

3. Comparisons are made of the results so obtained with those obtained by other methods, and the colour identity method is shown to give the correct result for melting platinum.

4. Formulæ are deduced, based on the fundamental theories of energy radiation and the sensitivity of the eye, connecting the temperature of carbon and tungsten filaments with their lumens per watt, and it is shown that these expressions hold from the lowest to the highest values of lumens per watt.

5. It is shown that the colour identity method of determining filament temperatures is practically independent of the cooling at the ends of the filaments of ordinary lamps.

6. An explanation is given of the principal factors and limitations of the colour identity method in which it is shown that accurate results should be obtained so long as the bodies under consideration act as " grey " bodies throughout the visible spectrum, and that there will be a tendency to error to the extent that they depart from the grey body condition in the *visible spectrum*.

7. The colour of the radiation from melting platinum is shown to be the same as that from a carbon filament lamp operating at $2 \cdot 6_5$ lumens per watt, or $4 \cdot 7_5$ watts per mean spherical candle, or approximately $3 \cdot 8$ watts per mean horizontal candle.

The work described in this Paper is not in the nature of a complete investigation of the subject. It had for its original

object the determination of the colour of the light from molten platinum under the open radiation conditions which prevail in the realisation of the Violle standard of light   A good primary standard of light must not only be constant and accurately reproducible, but the colour of its light should approximate to that of the sources which are ordinarily used in practice, so that large colour differences will not be involved in the photometric measurements for which such a standard is used.   The object for which the investigation was started was completed over two years ago, but the progress of the work indicated some unexpected phenomena which it was intended to investigate further.   Pressure of other work has up to now prevented this being done, and the authors desire at this stage to publish this preliminary note on the subject.   The accuracy of the work is the accuracy of preliminary experiments in which all reasonable precautions have been taken.   Values given for temperature certainly have not an absolute accuracy of more than 1 or 2 per cent., but the methods described are capable of a higher precision, and this will undoubtedly be attained in the fuller investigation which it is intended to undertake.

General Discussion.

Optical pyrometry is almost exclusively concerned with the *intensity* of the light emitted by a luminous body in any given wave-length.   The colour of the light thus dealt with is fixed by the wave-length or wave-lengths chosen for the measurements, and colour differences do not occur.

In ordinary photometry the sum of the intensities of the light emitted by a source in all wave-lengths over the visible spectrum is compared against the sum of the intensities of the light emitted by another source.   The radiation from each source has thus a composite colour whose characteristics will depend on the relative intensity of the light in each wave-length.   Although both sources may radiate according to the law of a black body, if there should be a difference of temperature between them the composite colour or hue of the radiations from the two bodies will differ, and it becomes necessary to compare intensities which are not of the same colour.   For most solid radiators the colour of the light is a perfectly definite quality, and forms a criterion of the state of incandescence of such bodies.   Most bodies are more or less selective in their radiation, but there is a certain group, consisting mainly of metallic substances, which although appa-

rently selective in favour of the visible spectrum *as a whole*, emit light throughout that spectrum without any appreciable deviation from the distribution to be found in the visible spectrum of a black body.* For instance, consider a tungsten filament adjusted to a suitable temperature, and compared spectrophotometrically against a carbon filament. The one is mainly selective in favour of the visible spectrum as a whole, and the other acts in this respect as a black body. The spectrophotometer, dealing only with the visible spectrum, cannot detect any relative difference between the two at different wave-lengths throughout the portion of the spectrum with which it deals, and thus a comparison of the total visible radiation is possible with an ordinary photometer, exact identity of colour being obtainable. That is to say, these substances virtually radiate as " grey " bodies, as far at least as the visible spectrum is concerned, and it is this close approximation, to grey body radiation in the visible spectrum which lies at the root of the method discussed in this Paper. Hence, identity of colour can be obtained not only when comparing one tungsten lamp against another, but also when comparing a tungsten lamp against a black body. If the temperature of these bodies is pushed to an extreme value a very slight difference of colour is perceptible at the point where the colour balance is closest, but such differences are too small to prevent an observer obtaining consistent results in judging the colour balance between two radiations.

A comparison of colour is made similarly to photometric comparisons of intensity. The current in the comparison lamp is varied so that the colour of the light fluctuates on both sides of the mean, first inclining to be redder and then to be bluer than the light from the test source. The current in the lamp is then readily determined at which the observer judges the colour balance to occur. It must be remembered that in these comparisons it is the hue of so-called white light which is under consideration, and not that of spectral or other colours.

The colour identity method depends on the combined effects of the light emitted in all wave-lengths in the visible region. If the intensity is relatively greater at the red end than at the blue end, the hue of the resulting radiation will tend to be red, and vice versa. The radiation from a black body at 1,750 C.

* Coblentz, " Radiation Constants of Metals," " Bull." B.S., Vol. V. p. 359. Hyde, " Selective Emission of Incandescent Lamps," " Trans.," Ill. Eng. Soc., 1909.

has a definite hue depending on the relative proportions of the energy in the red, green and blue regions, and any other radiator emitting light in the same *relative proportions* will have the same hue of radiation, no matter what the absolute intensity of the radiations. Thus, it is that the radiation from a grey body will be identical in hue with that of a black body, and compared on the colour identity basis the grey body will be given its true temperature. The optical pyrometer, on the other hand, only takes account of the relative *intensities* of the light from the black and grey bodies, and, therefore, estimates the temperature of the grey body at a value far below its true temperature. It follows, therefore, that the measure of the accuracy of the colour identity method is the extent to which bodies radiate as *grey* (*or black*) *bodies throughout the visible spectrum.*

Throughout this Paper the usual conception of a grey body is adopted—*i.e.*, one which, at any temperature, does not radiate as much energy in the various wave-lengths as a black body at the same temperature, but in any wave-length the intensity per unit area of the surface is a constant fraction of that of the black body in the same wave-length.

By a selective body is meant one in which the amounts of energy radiated in the various wave-lengths throughout the whole spectrum do not bear a constant proportion to those in the same wave-lengths for a black body at the same temperature.

Section 1 of this note deals with the establishment of electric sub-standards of colour, which are intended to serve for defining the colour of the radiation from any incandescent bodies compared against them, and so to fix the temperature of such bodies in terms of the temperature of a black body whose radiation is identical with theirs in colour.

Section 2 gives the determination of " colour identity " temperatures of carbon and tungsten glow lamps when burning at different efficiencies, and contains expressions for such efficiencies in terms of temperature based on Wien's equation for intensity of energy distribution and Nutting's equation for the sensitivity of the human eye.

Section 3 discusses the accuracy of such determinations, and deals with the " colour identity " temperature of platinum at the melting point, showing that even for a selective radiator such as platinum this temperature is a measure of the true

temperature of the platinum filament, although it is glowing
under open radiation conditions. Filament temperatures for
carbon and tungsten (vacuum and gas-filled) are also discussed.

Section 4 deals with the colour of the radiation from molten
platinum in relation to the practical usefulness of the Violle
standard of light.

1. *Electric Sub-standards of Colour for the Determination of Temperature.*

In spite of the fact that the device of colour comparison by
means of a Lummer Brodhun photometer has been used for
many years by various observers for obtaining equality of
efficiency of glow lamps of the same type, it is not generally
realised how easily and with what precision such colour com-

Fig. 1.—Diagram showing Arrangement of Apparatus used to obtain
the Relation between the Current in Carbon and Tungsten
Filament Lamps, and the Temperature of a Black Body at
Equality of Hue of their Radiations.

parisons can be made. Morris,* Stroud and Ellis employed
this method in 1907, and extensive use has been made of it for
investigating selectivity and other properties of radiating
substances by E. P. Hyde,† with whom Cady and Middlekauff
have sometimes collaborated. Hyde,‡ in discussing the ques-
tion of colour identity and temperature (p. 40, *loc. cit.*), showed
that a colour match with a black body might be regarded as
indicating that the temperature of the black body was at

* Morris, Stroud and Ellis, " The Electrician," Vol. LIX., p. 584.
† Hyde, Cady and Middlekauff, " Selective Emission of Incandescent
Lamps," Ill. Eng. Soc., New York, Vol. IV., 1909, p. 334. Hyde, " Physical
Characteristics of Luminous Sources," Lectures, John Hopkins University,
1910. Hyde, " Radiation Laws for Metals," " Astrophys. Journ.," Vol.
XXXVI., 1912, p. 89.
‡ Hyde, " The Physical Production of Light," " Journ." Franklin Inst.,
Vol. CLXX., 1910.

least as high or higher than that of the body compared against it, but he expressed the opinion (p. 39) that under the condition of colour identity two different radiators although with continuous spectra would not be at the same temperature.

A black body furnace electrically heated and capable of being raised to a temperature of 2,200°C., with a clear internal atmosphere, was kindly put at the disposal of the authors by Dr. J. A. Harker, F.R.S. The very excellent arrangements of this furnace need not be explained in detail here. The apparatus is shown diagrammatically in Fig. 1. A is a black body kept clear of fumes by a stream of nitrogen admitted at K.
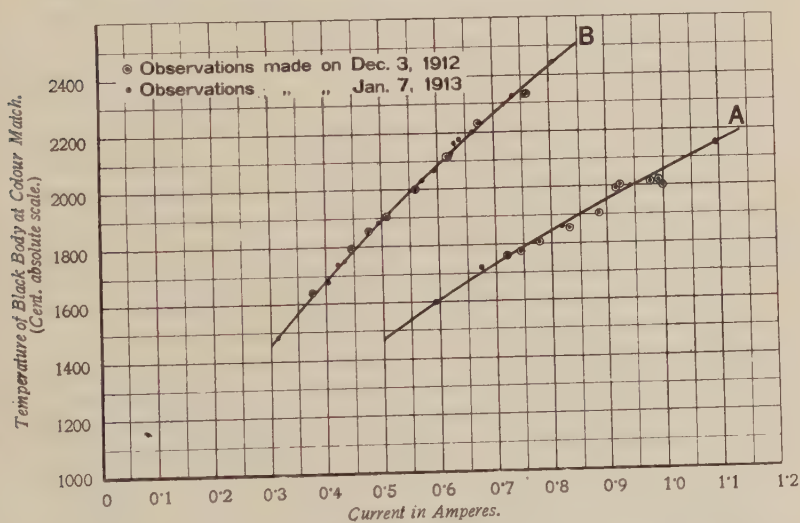

FIG. 2.—CARBON (A) AND TUNGSTEN (B) COLOUR STANDARDS.

Curves connecting the current in the lamps with the temperature of the black body whose radiation is identical in colour with that of the lamps.

B is a diaphragm with glass window, which permits only light from the centre of the incandescent surface to pass down the photometer bench, and J are screens to cut off extraneous light. C is a carefully seasoned electric lamp, the current through whose filament can be accurately measured by means of ammeter D. Between the two at E is a Lummer Brodhun photometer head. This photometer is not used to compare the *intensities* of the two sources of light A and C, but to determine when the hue of their radiations is identical. Two optical pyrometers were used of the Siemens and Féry types, by means

of which the temperature of the black body was determined
before and after each colour determination by the photometer.
Complete determinations were made on two separate occasions
and the mean result taken—every photometer and pyrometer
setting being made on each occasion by two observers. The
furnace was first run at a relatively low temperature, and the
electric heating adjusted so that it would remain at a constant
temperature sufficiently long for both photometric and pyro-
metric readings to be taken. The hue of the light from the
electric lamp C was varied by means of rheostat H until there
was exact identity of colour in the photometer. In doing this
it is, of course, necessary to place the photometer so that there
is also equality of brightness.

Two or three settings were made by each observer before and
after which the pyrometers were read. The same process was
followed in a series of increasing temperatures up to about
2,200° C.—the maximum temperature to which the furnace
was carried in these experiments. Two electric lamps were
calibrated in this way, one having a carbon and the other a
tungsten filament. These two lamps calibrated in the above
manner form intermediate standards of colour against which
any glow lamp can be matched, and the temperature of the
black body fixed to which the colour of its light corresponds.
The temperatures so determined are on the optical scale, using
pyrometers for which the dominant wave length is $\lambda = 0.650\mu$.

Fig. 1 shows the curve connecting current in the lamps and
the temperature of the black body for colour identity in the case
of each of these lamps. It was found that the sensitivity of the
process of colour matching is more than equal to that of tem-
perature measurement by optical pyrometers.

Duplicates of these two lamps were then made for use in
experiments where their continued employment at the higher
efficiencies might affect their constancy.

2. "Colour Identity" Temperatures Corresponding with Different Efficiencies.

It is common for the specific consumption of glow lamps to
be stated in terms of the watts per mean horizontal candle. The
only rigorous way, however, is to state it in terms of watts per
mean spherical candle or in lumens per watt. Throughout
this Paper the latter designation is used, representing as it does
the true measure of the ratio of the total light emitted to the

power supplied.   In the table of results the approximate watts per mean horizontal candle of the glow lamps is also given, since this is the more familiar designation, but it is not rigorous on account of the varying ratios of mean horizontal to mean spherical candle-power to be found in different lamps.   It is to be noted that in the determination of the total light emitted from the glow lamps the light which is obscured by the cap of the lamp is counted as being radiated and not absorbed, but in any case this light is less than 1 per cent. of the total for filaments of ordinary form.

The objects of the measurements are :—

(a) to find the relation between the various values of lumens per watt for tungsten and carbon filament lamps and the corresponding temperatures of a black body on the basis of colour identity ;

(b) to ascertain to what extent the temperature so measured represents those of the principal parts of the glowing filaments, having regard to the cooling effect of the filament supports ;

(c) to find laws connecting lumens per watt and corresponding "colour identity" temperature for tungsten and carbon filaments.

Evidence is given later in the Paper to show that there seems to be justification for the assumption that the colour identity method at any rate in certain cases gives within narrow limits a measure of the true filament temperature.   This assumption is, therefore, made in what immediately follows here, and it will be seen that the results which follow from this assumption whilst not proving its validity, are in agreement with those of Forsythe, who determined temperatures by more orthodox methods.

(a) A number of carbon and tungsten filament lamps were selected for the measurements.   The carbon lamps had both flashed and unflashed filaments.   The tungsten lamps had squirted and drawn filaments of different diameters and lengths, so that the effect of the cooling of the ends by the leading-in wires if appreciable might be observed.

All the lamps were measured for lumens per watt at different voltages up to the highest they were capable of standing without deterioration.   They were then compared for identity of colour against the colour standards, and in this way the temperatures of a black body were determined which corresponded with the various values of lumens per watt.   The results are given in Table 1., and plotted in Fig. 3.

TABLE I.

(Reduction factor = Mean spherical candle-power divided by mean horizontal candle-power.)

Carbon Filament Lamp (flashed) No. 1.
100 volts, 16 candles.

Reduction Factor = 0.85₅.

Volts.	Watts per mean horizontal candle.	Lumens per watt.	Temperature of black body at identity of colour (Cent. absolute).
$49{\cdot}2_5$	$111{\cdot}_0$	$0{\cdot}09_7$	1,515
$59{\cdot}5_0$	$38{\cdot}_0$	$0{\cdot}28_4$	1,640
$72{\cdot}7_0$	$13{\cdot}_9$	$0{\cdot}77_5$	1,755
$82{\cdot}7_0$	$7{\cdot}9_5$	1·35	1,865
$92{\cdot}5_0$	$5{\cdot}0_3$	2·14	1,960
103·0	$3{\cdot}7_7$	2·85	2,055
110·0	$2{\cdot}5_9$	4·14	2,120
130·0	$1{\cdot}5_6$	6·85	2,250
135·0	$1{\cdot}3_7$	$7{\cdot}7_7$	2,300
140·0	$1{\cdot}2_1$	$8{\cdot}8_5$	2,325

Carbon Filament Lamp (flashed) No. 3.
100 volts, 16 candles.

Reduction factor = 0.85.

Volts.	Watts per mean horizontal candle.	Lumens per watt.	Temperature of black body at identity of colour (Cent. absolute).
65	$19{\cdot}_5$	0·55	1,720
70	$13{\cdot}_8$	$0{\cdot}77_5$	1,775
75	$10{\cdot}_0$	$1{\cdot}06_5$	1,835
80	$7{\cdot}6_4$	$1{\cdot}40_5$	1,890
85	$6{\cdot}0_4$	$1{\cdot}77_5$	1,935
90	$4{\cdot}8_6$	2·20	1,985
100	$3{\cdot}3_4$	3·21	2,070
105	$2{\cdot}7_9$	3·83	2,115
107	$2{\cdot}6_3$	4·07	2,130
115	$2{\cdot}0_4$	5·25	2,200
120	$1{\cdot}8_1$	5·91	2,240
130	$1{\cdot}4_3$	7·50	2,310
135	$1{\cdot}2_5$	8·58	2,345

Tungsten Filament Lamp (Drawn) No. 6.
115 volts, 30 watts.

Reduction factor = 0.79.

Volts.	Watts per mean horizontal candle.	Lumens per watt.	Temperature of black body at identity of colour (Cent. absolute).
38·2	$21{\cdot}_7$	0·46	1,640
47·4	$11{\cdot}_1$	0·90	1,755
57·8	$6{\cdot}3_2$	1·57	1,865
68·7	$4{\cdot}0_2$	2·47	1,960
93·2	$1{\cdot}9_2$	5·17	2,145
120·5	1·1	9·00	2,325

Tungsten Filament Lamp (Squirted) No. 7.
105 volts, 32 watts.

Reduction factor = 0.79.

Volts.	Watts per mean horizontal candle.	Lumens per watt.	Temperature of black body at identity of colour (Cent. absolute).
38·8	$15{\cdot}_6$	0·63	1,640
48·2	$9{\cdot}3_4$	1·06	1,755
57·2	$5{\cdot}4_1$	1·84	1,865
67·0	$3{\cdot}5_8$	2·78	1,960
90·5	$1{\cdot}7_6$	5·66	2,145
117·0	$1{\cdot}0_8$	9·54	2,325

Carbon Filament Lamp (flashed) No. 2.
100 volts, 16 candles.

Reduction Factor = 0.89.

Volts.	Watts per mean horizontal candle.	Lumens per watt.	Temperature of black body at identity of colour (Cent. absolute).
65	$20{\cdot}_2$	$0{\cdot}55_5$	1,715
70	$14{\cdot}_1$	0·80	1,775
75	$10{\cdot}_2$	1·09	1,835
80	$7{\cdot}7_0$	1·45	1,890
85	$6{\cdot}1_3$	1·84	1,935
90	$4{\cdot}8_2$	2·33	1,980
100	$3{\cdot}2_1$	3·43	2,075
105	$2{\cdot}6_9$	4·17	2,120
107	$2{\cdot}5_3$	4·44	2,135

Carbon Filament Lamp (flashed) No. 4.
200 volts, 16 candles.

Reduction factor = 0.86₅.

Volts.	Watts per mean horizontal candle.	Lumens per watt.	Temperature of black body at identity of colour (Cent. absolute).
140	$18{\cdot}_2$	0·60	1,710
150	$12{\cdot}_7$	0·85	1,770
160	$9{\cdot}0_0$	1·18	1,835
180	$5{\cdot}2_0$	2·09	1,955
200	$3{\cdot}2_7$	3·41	2,070
210	$2{\cdot}6_5$	4·05	2,120
220	$2{\cdot}2_0$	4·95	2,165

Carbon Filament Lamp (unflashed) No. 5.
200 volts, 16 candles.

Reduction factor = 0.82₅.

Volts.	Watts per mean horizontal candle.	Lumens per watt.	Temperature of black body at identity of colour (Cent. absolute).
140	$22{\cdot}_2$	0·47	1,710
150	$15{\cdot}_2$	0·68	1,775
160	$10{\cdot}_9$	0·95	1,835
180	$61{\cdot}_4$	1·68	1,940
200	$3{\cdot}7_8$	2·74	2,050
210	$3{\cdot}0_8$	3·36	2,095
220	$2{\cdot}5_4$	4·08	2,140

Tungsten Filament Lamp (Squirted) No. 8.
105 volts, 30 watts.

Reduction factor = 0.78₅.

Volts.	Watts per mean horizontal candle.	Lumens per watt.	Temperature of black body at identity of colour (Cent. absolute).
37	$14{\cdot}_7$	0·72	1,710
40	$11{\cdot}_6$	0·85	1,745
45	$7{\cdot}8_5$	1·26	1,800
50	$6{\cdot}1_6$	1·62	1,860
55	$4{\cdot}7_3$	2·10	1,900
60	$3{\cdot}7_8$	2·61	1,960
65	$3{\cdot}0_6$	3·28	2,010
70	$2{\cdot}6_0$	3·81	2,055
75	$2{\cdot}2_2$	4·47	2,095
80	$1{\cdot}9_1$	5·21	2,135
90	$1{\cdot}4_0$	6·65	2,215
100	$1{\cdot}2_0$	8·19	2,285
105	$1{\cdot}0_9$	9·06	2,320

TABLE I.—*Continued.*

Volts.	Watts per mean horizontal candle.	Lumens per watt.	Temperature of black body at identity of colour (Cent. absolute).	Volts.	Watts per mean horizontal candle.	Lumens per watt.	Temperature of black body at identity of colour (Cent. absolute).
Tungsten Filament Lamp (Squirted) No. 9. 105 volts, 60 watts.				*Tungsten Filament Lamp (Drawn) No. 10.* 100 volts, 15 watts.			
Reduction factor = 0·78₅.				*Reduction factor* = 0·78₅.			
40	$12\cdot8$	$0\cdot77_5$	1,710	45	$8\cdot6_0$	$1\cdot17$	1,800
45	$9\cdot8_2$	$1\cdot01$	1,775	50	$6\cdot1_5$	$1\cdot59$	1,860
50	$7\cdot3_4$	$1\cdot35$	1,835	55	$4\cdot8_0$	$2\cdot05$	1,915
55	$5\cdot7_0$	$1\cdot74$	1,880	60	$3\cdot8_8$	$2\cdot55$	1,960
60	$4\cdot5_3$	$2\cdot18$	1,920	65	$3\cdot2_0$	$3\cdot09$	2,015
65	$3\cdot7_1$	$2\cdot67$	1,970	70	$2\cdot6_6$	$3\cdot71$	2,055
70	$3\cdot1_2$	$3\cdot17$	2,015	75	$2\cdot2_6$	$4\cdot36$	2,105
75	$2\cdot6_5$	$3\cdot73$	2,055	80	$1\cdot9_5$	$5\cdot10$	2,140
80	$2\cdot2_6$	$4\cdot37$	2,100	90	$1\cdot5_0$	$6\cdot75$	2,225
90	$1\cdot7_4$	$5\cdot70$	2,175	100	$1\cdot2_1$	$8\cdot13$	2,290
100	$1\cdot3_9$	$7\cdot12$	2,245	105	$1\cdot1_1$	$9\cdot02$	2,325
105	$1\cdot2_5$	$8\cdot06$	2,280	110	$1\cdot0_0$	$9\cdot88$	2,360
110	$1\cdot1_4$	$8\cdot66$	2,315	115	$0\cdot91_5$	$10\cdot8_2$	2,400
115	$1\cdot0_4$	$9\cdot51$	2,340	120	$0\cdot83_5$	$11\cdot8_3$	2,430
120	$0\cdot94_5$	$10\cdot4_5$	2,375	125	$0\cdot77_5$	$12\cdot7_6$	2,455
125	$0\cdot88_5$	$11\cdot2$	2,395	130	$0\cdot72_0$	$13\cdot7_3$	2,485
130	$0\cdot82_0$	$12\cdot1$	2,435	135	$0\cdot66_5$	$14\cdot8_5$	2,505
135	$0\cdot76_0$	$13\cdot0_5$	2,460				
Tungsten Filament Lamp (Drawn) No. 11. 200 volts, 20 watts.				*Tungsten Filament Lamp (Drawn) No. 12.* 230 volts, 60 watts.			
Reduction factor = 0·78₅.				*Reduction factor* = 0·78₅.			
80	$13\cdot5$	$0\cdot73$	1,720	100	$9\cdot5_0$	$1\cdot04$	1,770
90	$9\cdot7_0$	$1\cdot02$	1,770	110	$7\cdot3_5$	$1\cdot34$	1,825
100	$7\cdot2_5$	$1\cdot36$	1,835	120	$5\cdot7_0$	$1\cdot74$	1,870
110	$5\cdot5_0$	$1\cdot80$	1,885	130	$4\cdot5_4$	$2\cdot17$	1,915
120	$4\cdot4_2$	$2\cdot23$	1,935	140	$3\cdot8_0$	$2\cdot60$	1,960
130	$3\cdot5_8$	$2\cdot75$	1,970	150	$3\cdot1_9$	$3\cdot10$	2,000
140	$3\cdot0_1$	$3\cdot28$	2,020	160	$2\cdot7_4$	$3\cdot60$	2,040
150	$2\cdot5_5$	$3\cdot87$	2,060	170	$2\cdot3_5$	$4\cdot1_9$	2,080
160	$2\cdot1_8$	$4\cdot4_8$	2,105	180	$2\cdot0_5$	$4\cdot8_1$	2,110
180	$1\cdot7_1$	$5\cdot8_5$	2,170	190	$1\cdot8_1$	$5\cdot4_6$	2,145
200	$1\cdot3_8$	$7\cdot2_5$	2,245	210	$1\cdot4_6$	$6\cdot7_8$	2,210
210	$1\cdot2_5$	$7\cdot9_0$	2,270	230	$1\cdot2_2$	$8\cdot0_8$	2,270
220	$1\cdot1_4$	$8\cdot6_6$	2,320	240	$1\cdot1_3$	$8\cdot7_2$	2,305
				260	$0\cdot96_5$	$10\cdot2$	2,360

In obtaining the higher temperature values for plotting on Fig. 3, it is very useful to make use of a " watt-temperature " curve. The carbon lamp, for instance, cannot with safety be run for long periods at temperatures in the region of 2,000°C. If the watts be plotted against temperatures obtained by the identity of colour method the resulting curve will be found to be a logarithmic, and no deviation whatever can be detected

from such logarithmic over the range between the highest and
the lowest observed values. This is illustrated in Fig. 4, in
which the logs of temperature and watts have been plotted for
both carbon and tungsten lamps Nos. 3, 5, 6, 8 and 12. It is
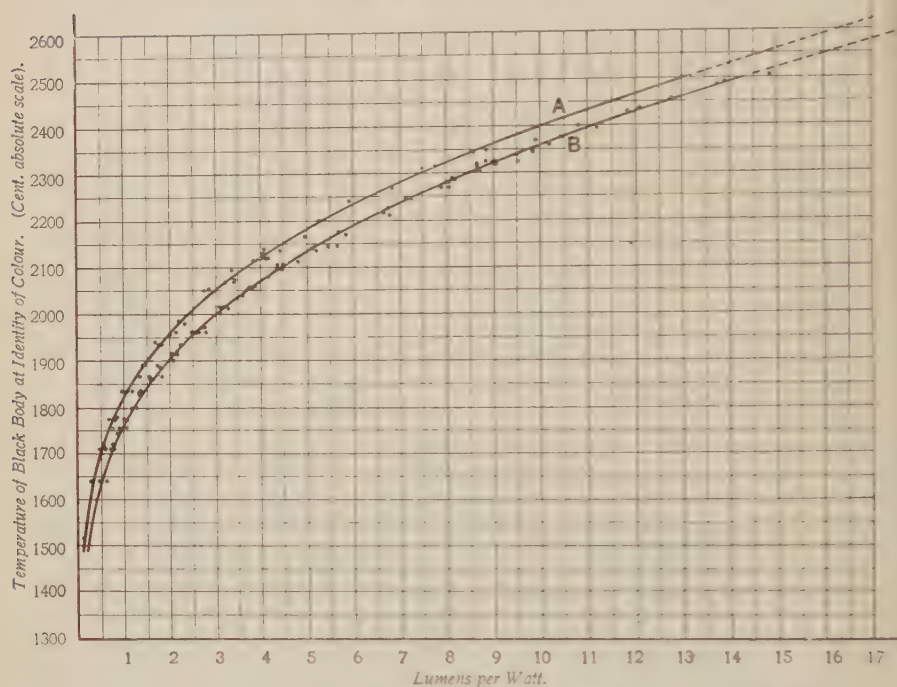

Fig. 3.—Ordinary 100 and 200-volt Carbon (Curve A) and Tungsten
(Curve B) Filament Vacuum Lamps.

Curves connecting lumens per watt of the lamps with the temperature of a
black body whose radiation is identical in colour with that of the lamps.

safe from a knowledge of the watts in any lamp to deduce
intermediate temperature values from such a curve, so that
the actual number of colour comparisons may be a minimum,
and the burning period of the colour standard reduced.

The watt-temperature relation is given, for carbon filament
lamps, by

$$W \propto T^{4 \cdot 58},$$

and for tungsten filament lamps by

$$W \propto T^{5 \cdot 1},$$

Considering again Fig. 3, the first point to notice is that no difference can be detected between the various carbon lamps tested or between the different tungsten lamps.   Whether the carbon filaments are flashed or unflashed, and the tungsten filaments squirted or drawn, appears to leave unaffected the relation between lumens per watt and the corresponding " colour identity " temperature of a black body for either of these types.

It will, therefore, be seen that all the results may be taken as
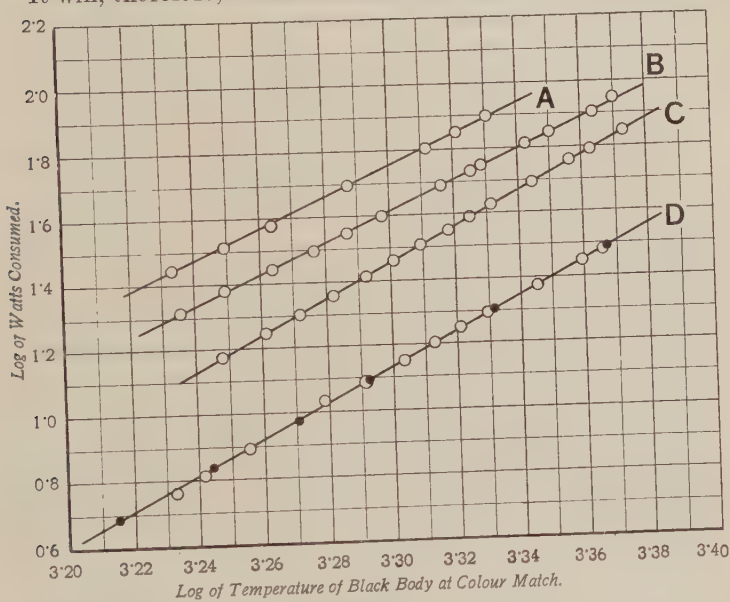
Fig. 4.

Curves connecting watts consumed by carbon and tungsten filament lamps, and the temperature of the black body whose radiation matches in colour that of the lamps.

A is for Lamp No. 5.   (Carbon filament.)
B   ,,   ,,   No. 3.   (,,   ,,   )
C   ,,   ,,   No. 12.   (Tungsten   ,,   )
D   ,,   ,,   Nos. 6 and 8. (,,   ,,   )

lying on two curves, one representing the carbon group and the other the tungsten group, and so closely do the points keep to the curves that very few observations lie more than 1 per cent. in temperature from the mean curve.

This implies that in all ordinary lamps of the same character (vacuum tungsten or carbon) the colour of the radiation from the whole filament, including the cooled ends, is the same for the same value of lumens per watt.   Therefore, to considerable

accuracy it may be said, that a knowledge of the lumens per watt of a lamp implies a knowledge of the temperature of a black body whose radiation is the same in hue as that of the lamp.

(b) The following considerations show to what extent a temperature, determined as above, may be regarded as the temperatures of the main glowing part of the filament. If there were no cooling at the ends of a filament it would be equally bright for the whole of its length. The cooling, however, as shown by Hyde, Cady and Worthing* is appreciable, but it must be remembered that the colour of the light is governed by the part of the filament giving off most light. The ends of the filament give off actually very little light because the amount emitted falls off according to a very high power of the temperature (T^{12} to T^{20}). Hence the effect of the dulled ends of the filament on the colour of the total light from it, is exceedingly small. In an actual case† the total light emitted below the point where the filament began to become measurably dull was only 5 per cent. of the whole, and a large percentage of this amount differs only very slightly in colour from the light emitted by the remainder of the filament.

The following measurements, Table II., were made of the total effect on the measured temperature due to end cooling by determining the "colour identity" temperature of the central portion of the filament only and comparing it against that of the whole filament, including the cooled ends.

TABLE II.

Lamp.	Volts.	Temperature of black body for colour identity.	
		Whole filament.	Centre of filament.
No. 11	200	2,245	2,255
	110	1,885	1,898
	54	1,556	1,570
No. 9	105	2,280	2,290
	60	1,920	1,930
	40	1,710	1,722
Motor headlight. 16 volts, 50 watts	...	1,486	1,508
	...	2,014	2,043
	...	2,240	2,285

* Amer. Ill. Eng. Soc. "Trans." 6, pp. 238-257.
† See Hyde, Cady and Worthing. Loc. cit.

Lamp No. 11 was an ordinary 200 volt, 20 watt tungsten lamp, and No. 9 was rated for 100 volts, 55 watts ; the difference of filament diameter and distance between supports were, therefore, as large as is usually met with in practice. The motor headlight filament was for 16 volts and 50 watts, and, therefore, represented an extreme case.

It will be seen from this that unless a very thick, short filament be taken with abnormal end cooling, the measured colour identity temperature will be that of the central bright portion of the filament, within about 1 per cent. Whilst in the extreme case of the headlight lamp it is of the order of 2 per cent. It is obvious that the cooling effect for carbon filament lamps is considerably less than for tungsten, and is, in fact, quite inappreciable.

It is thus clear that the colour identity method gives results which depend very closely on the temperature of the central portion of the filament. If it may be assumed that the method also gives the *true* temperature of lamp filaments, the figures in Table I. and Fig. 3 indicate the appreciable difference of efficiency existing between the carbon and tungsten lamps for the same temperature, and, therefore, establish the selectivity of the tungsten filament in favour of the shorter wave-lengths, a subject upon which much has been written, and which has been thoroughly investigated by Dr. E. P. Hyde.* This difference in efficiency would, if anything, be very slightly increased by taking into account the end cooling of the filaments, the tendency of which is to act in favour of the carbon lamp. Also, if the carbon filament is " greyer " than the tungsten filament in the visible region the apparent difference of efficiency will be increased.

(*c*) Referring to the curves shown in Fig. 3 and bearing in mind what has been said in the foregoing remarks, it becomes of interest to know if a relation connecting lumens per watt and temperature can be deduced from our knowledge of the phenomena involved, and especially to ascertain how nearly the experimental observations conform to such a relation deduced from theoretical considerations.

We have to consider, therefore, how the rate of dissipation of energy by a lamp filament, *i.e.*, the watts,† increases with a

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* *See* Hyde. *Loc. cit.*
† The rate at which energy is radiated is power and is spoken of hereafter as radiant power.

rise in temperature, and also how the eye estimates the rate at which this energy is radiated.

The eye is only sensitive to a small portion of this energy, i.e., that emitted in wave-lengths lying approximately between 0.3μ and 0.8μ. Further, the eye does not appreciate the intensity of the energy radiation in any wave-length over this limited range in direct proportion to the amount radiated, but weights it according to its own peculiar sensitivity to energy of that wave-length. This appreciation of power by the eye is expressed in lumens which may be defined as the measure of the appreciation of the eye for radiant power.

An expression must, therefore, be found connecting lumens and the temperature of the radiating body both in terms of the power distribution throughout the visible spectrum and of the sensitivity characteristics of the eye.

The theoretical investigation of the problem thus subdivides itself naturally into three distinct parts :—

(a) The rate of energy dissipation of the radiator at any temperature.

(b) The quantitative distribution of this radiant power throughout the spectrum at any temperature, with special reference to that range of the spectrum over which the energy stimulates the sense of vision.

(c) The relative capacity of equal amounts of radiant power in different wave-lengths for stimulating vision, this being necessarily referred to the average or normal human eye.

(a) *Relation Between Watts and Temperature.*—Attention has been already drawn to curves showing the relation between the rate of dissipation of energy by a lamp and its temperature as measured by the colour identity method. Many lamps of ordinary dimensions have been examined, and in all cases the results can be expressed by an equation of the form

$$\text{watts} \propto T^m \text{ or } (\log W = \log D + m \log T), \quad . \quad . \quad (1)$$

m being 4.5 to 4.6 for carbon lamps and 5.0 to 5.2 for tungsten lamps.

In no case has any appreciable deviation been observed from this logarithmic relationship for temperatures ranging from 1,700 deg. to 2,300 deg. abs.

This relationship is at once recognised as being identical in form with that ascribed to Stefan and Boltzman connecting the temperature and radiant watts of the ideal black body, m in the latter case being 4.0.

(b) *Distribution of Radiant Power throughout the Visible Spectrum.*—In the case of the ideal black body, the radiant power in any wave-length of the visible spectrum at any temperature below 3,000°C. can be expressed according to the well-known law of Wien

$$E = C_1 \lambda^{-n} e^{-\frac{C_2}{\lambda T}} \quad \ldots \ldots \ldots \quad (2)$$

E being the radiant power of wave-length λ at temperature T, C_1 and C_2 being constants, $n=5$ (for a black body).

Seeing that in the experiments described in the following section (3), in which the colour of the radiation of melting platinum was found to be identical with that of a black body operating at the same temperature, it is reasonable to assume that the power distribution, at least *over the range of the visible spectrum*, can be expressed by a formula of the above form. Lummer and Pringsheim found this condition to be closely fulfilled by radiators having the characteristics of platinum.

When the visible spectrum only is under consideration the values of C_1, C_2 and n can vary considerably without affecting the shape of the curve by an amount corresponding to a difference of temperature of 10°C. in the region of 2,000°C.

(c) *Sensitivity of the Eye to Energy of Different Wave-lengths.* By examining a large number of persons, Nutting* has obtained data connecting the wave-lengths of radiant energy and the luminous sensation produced per unit of power in that wave-length.

He expresses his results in the form

$$V_\lambda = V_m \left(\frac{\lambda m}{\lambda}\right)^a e^{a\left(1-\frac{\lambda m}{\lambda}\right)}. \quad \ldots \ldots \quad (3)$$

where V_m is the photometric value of a unit of power in the wave-length of maximum sensitivity λ_m, a is a constant, e is the base of Napierian logarithms.

For the luminous intensities ordinarily employed in photometry he gives $a=181$ and $\lambda_m=0.55\mu$.

Combining the expression for power distribution and the sensitivity curve for the eye (equations 2 and 3), we obtain for the photometric value of radiant power of wave-length λ, $E_\lambda V$, and for the photometric value of the whole of the radiant power

$$\int_0^\infty E_\lambda V_\lambda d\lambda. \quad \ldots \ldots \ldots \quad (4)$$

* " Bulletin " of the Bureau of Standards, Vol. V., p. 261, and Vol. VI., p. 337.

If the power distribution can be represented by

$$E_\lambda = P\lambda^{-n}e^{-\frac{Q}{\lambda T}}. \quad \ldots \quad \ldots \quad (5)$$

Nutting* shows that the above expression for the photometric value of radiant power reduces to

$$L = A\left(1 + \frac{B}{T}\right)^{-\rho} \quad \ldots \quad \ldots \quad (6)$$

where the photometric value of the total radiant power in lumens at a given temperature, T. is represented by L.

$$A = PV_m\lambda^a_m e^n(a\lambda_m)^{-\rho}\Gamma\rho.,$$
$$B = \frac{Q}{a\lambda_m},$$
$$\rho = n + a - 1.$$

It follows from what has been said above that the lumens radiated by carbon or tungsten filaments should be capable of being represented by an expression of the form of equation (6). Combining with this expression that for the watt-temperature relationship for the filament under consideration (equation 1), we get for the equation connecting lumens per watt and temperature

$$\frac{L}{W} = A_1\left(1 + \frac{B}{T}\right)^{-\rho}T^{-m};$$

or expressed for convenience in the logarithmic form

$$\text{Log}\frac{L}{W} = C - m \log T - \rho \log\left(1 + \frac{B}{T}\right). \quad \ldots \quad (7)$$

From the measured values of lumens watt and temperature, which are plotted in Fig. 3, the following values of the constants in the foregoing equation are found.

For carbon filaments

$$\text{Log}_{10}\frac{L}{W} = 21\cdot51 - 4\cdot58 \log_{10} T - 185 \log_{10}\left(1 + \frac{155}{T}\right) \quad (8)$$

and for tungsten filaments

$$\text{Log}_{10}\frac{L}{W} = 23\cdot31_3 - 5\cdot1 \log_{10} T - 185 \log_{10}\left(1 + \frac{155}{T}\right). \quad (9)$$

* Loc. cit.

The curves drawn in Fig. 3 are those derived from these equations, and it will be seen at once how nearly the observations fall on the curves ; in fact, it would hardly be possible to find a form of curve which would fit the observations better. The error in temperature rarely exceeds 2 per cent. and in most cases is considerably less than 1 per cent.—*i.e.*, within the possible error of the experiments. Further, it is shown later that a very large extrapolation of the tungsten curve by this formula indicates a value for the melting point of tungsten which is not inconsistent with that found by other observers. The formulæ indicate that the maximum attainable efficiency would occur in the region of 6,000°C., which is quite in accord with accepted theories.

The origin of the constants in equations 7, 8 and 9 should be particularly noted. The watts-temperature relationship for a lamp has been found to be of the form $W \propto T^m$ (equation 1), m being a constant which appears in equation (7). The constant ρ of equation (7) is equal to $n + a - 1$, where " $-n$ " is the index of λ in equation (2) of the " Wien " form, which is assumed to give the power distribution curve for the filament throughout the visible part of the spectrum, and where " a " is derived from Nutting's equation for the sensitivity of the eye and has the value of 181. $B = \dfrac{Q}{a\lambda_m}$ (*see* equation 6), where Q is the other constant in the assumed Wein equation for power distribution, " a " has the value as before of 181, and λ_m is the wave-length of the energy to which the eye is most sensitive, *i.e.*, $0 \cdot 55\mu$. Hence $B = \dfrac{Q}{99 \cdot 55}$. For a true black or grey body $m = 4$, $n = 5$, $\rho = 185 = 14{,}500$, and $B = 145 \cdot 0$ approximately.

Before leaving the consideration of these equations connecting lumens per watt and temperature, it is desirable to discuss one or two points which at first sight may appear to have an important bearing on the deductions that can be made from the foregoing results.

Firstly as regards " n " in equation (5) and " m " in equation (7). For a black body the value of " n " in equation (5) is 5, and it will also be 5 for a true grey body whose radiation in all wave-lengths bears a definite proportion to that of a black body. It will not necessarily be 5, however, for selective bodies, although, as in the selective bodies under consideration,

they appear to radiate very much like grey bodies over *the visible spectrum.*

In equation (7) the constant " m," which is derived directly from equation (1), can only be regarded as connected with " n " ($n = m + 1$) in equation (5) if the latter represents the distribution of power throughout the *whole* spectrum, and not merely in the visible spectrum. This latter is the assumption made in using equation (5) in this investigation, and the extent of the work described here does not justify the wider application of equation (5) to the whole spectrum for substances which do not behave as true black or grey bodies.

Lummer and Pringsheim*, investigating platinum, state that the distribution of power throughout the whole spectrum for platinum is given by the following equation :—

$$E_\lambda = a\lambda^{-6} e^{-\frac{15,600}{\lambda T}} \quad . \quad . \quad . \quad . \quad . \quad (10)$$

corresponding to the form for an ideal black body of

$$E_\lambda = C_1 \lambda^{-5} e^{-\frac{14,500}{\lambda T}} . \quad . \quad . \quad . \quad . \quad (11)$$

If this assumption were justifiable, the authors' values for tungsten work out very nearly the same as those given by Lummer and Pringsheim for platinum ; but, for the reasons just stated, too much significance must not be attached to this agreement.

Coblentz† expresses the opinion, based on an investigation of several metals, that " n " in equation (5) is not a constant, but is a function of wave-length and temperature.

There is nothing in the results discussed above which is inconsistent with either Lummer and Pringsheim's or Coblentz's suggestions.

Throughout this work the practical case has been dealt with of filaments mounted in exhausted globes in which there is undoubtedly some loss of watts and efficiency due to the cooling effect of the leading-in wires. If conclusions of a fundamental nature are to be drawn from the results obtained, it is necessary to know to what extent this cooling is likely to affect the constants given in equations (8) and (9).

In the work already referred to by Hyde and Cady, figures are given for the loss of watts by conduction at the ends of

* Lummer and Pringsheim, " Verhandlungen der Deutschen Phys. Gesell.,' pp. 23-25, 1899.

† " Bulletin " B.S., Vol. V., pp. 338-379, 1908-1909.

filaments, and columns I. to IV. of the following Table are from this Paper.

<p align="center">TABLE III.</p>

I. Lamp.	II. Watts mean horizontal candles.	III. Watt loss.	IV. Effi- ciency loss.	V. Lumens Watt.	VI. Temp. ° Abs.
Carbon 115 volts ...	3·1	2%	4%	3·4₅	2,085
,, ,, ...	18·0	3%	5%	0·5₉	1,730
Tungsten 115 volts, 60 watts	1·25	4%	7%	8·0	2,280
,, ,,	11·0	8%	16%	0·9₁	1,745

These losses are calculated for different efficiencies as a percentage of the watts which would be required to maintain the filament throughout its whole length at the temperature of its midpoint, assuming no loss by conduction.

In columns V. and VI. are tabulated the values of lumens per watt calculated from Column II. using average values for the reduction factors of the types of lamp under consideration, and the corresponding temperatures taken from the curves in Fig. 3.

The difference between the temperature corresponding to the colour of the light radiated from the *centre* of the filament of any lamp of the above types, and that radiated from the *whole* filament are given in Table II. The following results are obtained by using the values in Tables II. and III. for ascertaining what would be the behaviour of the filaments used in this investigation had there been no cooling.

<p align="center">Carbon Lamps.</p>

In ordinary lamps the watts and temperature are connected by the relation

$$\mathrm{Log_{10}} W = C_1 + 4\cdot58 \log_{10} T \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Allowing for the watt loss as per column III., Table III., due to conduction, the watt-temperature relation for a filament kept at uniform temperature throughout its length and having no conduction losses is :

$$\mathrm{Log_{10}} W_1 = C_1 - 0\cdot191 + 4\cdot63_5 \log_{10} T. \quad . \quad . \quad (13)$$

Likewise for ordinary lamps the relation between lumens per watt and temperature is expressed by

$$\mathrm{Log_{10}} \frac{L}{W} = C_2 - 4\cdot58 \log_{10} T - 185 \log_{10}\left(1 + \frac{155}{T}\right). \quad . \quad (14)$$

Allowing in a similar manner for the efficiency losses (column IV., Table III.), the lumens per watt and temperature relation for the ideal filament is given by

$$\text{Log}\,\frac{L}{W}=C_2+0.199-4.63_5\,\log T-185\,\log\left(1\pm\frac{155}{T}\right). \quad (15)$$

Tungsten Lamps.

Similarly the equations for tungsten filaments are changed when cooling is allowed for, from

and $$\text{Log}\,W=C_3+5.1\,\log T, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

$$\text{Log}\,\frac{L}{W}=C_4-5.1\,\log T-185\,\log\left(1+\frac{155}{T}\right) \quad . \quad . \quad (24)$$

to

and $$\text{Log}\,W=C_3-0.700+5.3\,\log T, \quad . \quad . \quad . \quad . \quad . \quad (25)$$

$$\text{Log}\,\frac{L}{W}=C_4+0.703-5.3\,\log T-185\,\log\left(1-\frac{155}{T}\right) \quad (26)$$

and the same phenomenon is observable as for the carbon filament lamps.

By comparing equations (13) and (15) and (25) and (26) respectively and remembering that (13) and (25) deal only with the *watt* relationship, omitting lumens altogether, it will be seen that practically all the change produced by cooling is in the watts. That means that the loss of lumens at the ends of a filament is virtually equal to the gain in lumens due to the centre of the filament (as shown in Table II.) being at a slightly higher temperature than that ascribed by the colour identity method to the filament as a whole.

The fact that temperatures based on the colour identity method and covering so wide a range are thus found to fit in so well with fundamental theory seems to afford presumptive evidence that the colour identity method gives values of temperature which are accurate at least from the relative point of view, and affords further experimental support for the conclusions which are arrived at on page 233 as a result of a general consideration of the phenomena underlying colour identity.

3. *Temperature of Lamp Filaments and of Melting Platinum.*

The foregoing experiments assume that a temperature ascribed to a filament by comparing the hue of its total radiation with that of a black body of known temperature approxi-

mates to the true temperature of the filament although the latter is glowing under open radiation conditions. If this approximation can be shown to be a very close one, the method might be of considerable use in certain branches of practical pyrometry. In what follows enough evidence is given of the correctness of the temperatures determined by the colour identity method, to justify the assumption for certain substances and to warrant a more complete investigation of the subject.

As regards the determination of filament temperature by previous observers, only three sets of determinations, those of Forsythe,* Von Pirani and Meyer† and Langmuir‡ are given in comparable form.

In some other determinations no mention is made of the watts per candle or lumens per watt of the lamps tested. In others "black body" temperatures and not true temperatures are given of the filaments radiating in the open.§

In Table IV. the results obtained by the authors are compared with those of the above-mentioned observers. Both Forsythe and Von Pirani only measured the mean horizontal candle-power of their lamps, and a reduction factor of 0·85 for carbon and 0·79 for tungsten has been assumed in both cases for the ratio $\dfrac{\text{M.S.C.P.}}{\text{M.H.C.P.}}$ It will be seen that the authors' results agree very closely with those of Forsythe, and it should further be noted that whilst agreeing with Forsythe, who used the usual optical methods, in the case of comparatively non-selective carbon filaments, they also virtually agree with his results for tungsten, although the latter is admittedly selective.

Von Pirani and Meyer found values of true temperature which are appreciably higher than Forsythe's and, therefore, also higher than by the colour identity method given here. They are given in column IV. of Table IV., the values being taken off a curve through Von Pirani and Meyer's values and

* "Phys. Rev." Vol. XXXIV., May, 1912.
† "E.T.Z.," 1912, May 2, p. 457 and July 11, p. 725.
‡ "Proc." of Amer. Inst. of Elect. Engineers. Vol. XXXII., p. 1895.
§ Dr. H. Lux, "E.T.Z.," May 28, 1914, gives tables connecting tempera-
ture and watts per mean spherical candle of tungsten lamps. The
values given in the table would appear to approximate to true temperatures,
but the method described for determining the temperatures is that ordin-
arily used for obtaining black body temperature. Without further infor-
mation of the methods used by Dr. Lux for the determination of true tem-
perature, a useful comparison with readings of other observers is difficult to
make.

reduced to the same basis of lumens per watt. A considerable amount of this difference appears to be accounted for by the use by Von Pirani of a temperature scale which gives the melting point of platinum at 1,790°C.

TABLE IV.

Type of lamp.	Lumens per watt.	True Temperature of Filament °C.		
		Forsythe.	Pirani and Meyer.	The Authors.
Tungsten	$8 \cdot 0_5$	1,980	2,069	2,010
,, ...	$8 \cdot 1_5$	1,982	2,072	2,014
,, ...	$8 \cdot 4_4$	2,008	2,084	2,027
,, ...	$8 \cdot 8_3$	2,020	2,100	2,041
,, ...	$9 \cdot 0_6$	2,025	2,109	2,051
,, ...	$8 \cdot 8_8$	2,035	2,101	2,044
,, ...	$9 \cdot 3_6$	2,040	2,121	2,063
Carbon	$3 \cdot 5_2$	1,820	1,935	1,818
,, ...	$3 \cdot 9_5$	1,847	1,966	1,846
,, ...	$3 \cdot 9_4$	1,843	1,965	1,845

The lumens per watt are obtained from the values given by the authors of watts per candle, by assuming ratios of 0·79 and 0·85 respectively for the reduction factors $\left(\dfrac{\text{M.S.C.P.}}{\text{M.H.C.P.}} \right)$ for tungsten and carbon lamps.

The value of 0·9 is taken for the ratio of the Hefner to the British units of candle-power.

Both Von Pirani and Forsythe determined black body temperatures and added an amount depending on certain assumptions in order to get true temperatures. It is not possible to correct Von Pirani and Meyer's figures to make them comparable with those based on a temperature scale which gives the more usually accepted melting point for platinum, but it is clear that if this could be done Von Pirani and Meyer's figures would agree much more closely with Forsythe's. Langmuir does not give details of how he obtained his temperature values, since his Paper was not directly concerned with the measurement of temperature. His results differ by about 2 per cent from the authors' values obtained by extrapolating the curve shown in Fig. 3 of this Paper, using the formula given on page 246.

In order to ascertain if the colour identity method is correct for substances other than carbon it is necessary to know the true temperature of some glowing filament. The melting point of platinum is now very generally accepted as (1750 ± 20)°C.* If a filament of platinum could be gradually raised to the melting point by an electric current and compared at its

* Burgess-Le Chatelier. "Measurement of High Temperatures," p. 492.

melting point against one of the colour standards, the " colour identity " temperature of the platinum could be fixed at the melting point and compared with the known melting point of platinum. Platinum is admittedly a selective body. In addition, when radiating in the open its black body temperature determined by the ordinary optical methods ($\lambda = 0.650\mu$) is some 200°C. lower than the true temperature, and as the colour comparison would be made under open radiation conditions, the experiment should be a crucial one for proving if the colour identity method gives true temperatures.

The only precaution necessary is to take a fair length of platinum wire and use only the central portion so that the cooling of the ends of the wire by the leading-in terminals shall not influence the determination. Lengths of No. 25 gauge wire were used, 13 cm. long, of which all but the centre 5 cm. was screened off. Simultaneous comparisons were made with both carbon and tungsten colour standards. The current in the platinum wire was slowly raised and that in the colour standards increased so that identity of colour was always maintained in the photometer up to the melting point of the platinum. The colour standards were arranged each with a photometer head on either side of the platinum wire, and no difficulty was found in maintaining colour identity to the point at which the platinum melted.

Table V. gives the results of all the 15 determinations.

TABLE V.

Experiment.	Temperature of platinum at melting point by colour identity method °C.	
	With carbon filament colour standard.	With tungsten filament colour standard.
1	1,752°C.	1,765°C.
2	1,746°C.	1,770°C.
3	1,727°C.	1,751°C.
4	1,737°C.	1,765°C.
5	—	1,747°C.
6	1,761°C.	1,784°C.
7	1,769°C.	1,779°C.
8	1,755°C.	1,784°C.
9	1,763°C.	1,782°C.
10	1,759°C.	1,782°C.
11	1,727°C.	1,765°C.
12	1,737°C.	1,769°C.
13	1,747°C.	1,782°C.
14	1,764°C.	1,789°C.
15	1,757°C.	1,789°C.
	Mean 1,750°C.	1,773°C.

The mean result gives the melting point of platinum as 1,750°C. by the carbon filament lamp and 1.770°C. by the Tungsten lamp, a result so near to the accepted value of 1,750°C.* as to afford strong evidence of the reliability of the colour identity method. It is intended later to repeat this experiment with other metals such as nickel, iridium or rhodium, using a neutral atmosphere to surround the incandescent wires. The difference of 1 per cent. in the temperatures given respectively by the carbon and the tungsten colour standards must not be assigned too much weight. Although the method is capable of a greater accuracy than this, it is not claimed that the determinations described here are correct to 1 per cent.

Comparing again the usual optical methods and the colour identity method of estimating temperature, it is worth while to see what is the explanation of the phenomenon which has been described in this Paper. The factor of chief interest is, that if a black body at 1,750°C. radiates towards one side of a photometer and platinum at the melting point (1.750°C.) towards the other, there will be identity of hue on the two sides of the photometer, even though the platinum is operating under open radiation conditions. The hue of the total radiation of the platinum is, therefore, a measure of its true temperature.

If, on the other hand, a pyrometer be used to measure the temperature, first, of the black body and then of the melting platinum, it will give a value of 1,750°C. for the black body and about 1,550°C. for the platinum. The latter temperature will depend on the wave-length in which the measurements are made, but whatever the wave-length used the temperature given will be very much lower than the true temperature of the platinum.

Waidner and Burgess have given the melting point temperatures of platinum (black body) determined in three wavelengths as follows :—

Colour.	Wave-length.	Melting point.
Red	0.666μ	1,534°C.
Green	0.547μ	1,578°C.
Blue	0.462μ	1,610°C.

TABLE VI.

Examining the cases of a grey body and then of platinum, for which most data are available, it is possible to see from what

* Burgess-Le Chatelier, " Measurement of High Temperature," p. 492.

follows how closely the colour identity method will tend in practice to give the true temperature.

Curve A (Fig. 5*) shows the power distribution of a black body at 1,750°C. over the visible spectrum calculated from the Wien equation. Curve C is a curve for a grey body with a certain emissivity at the same *true* temperature. If the temperature of this grey body be measured with an optical pyro-
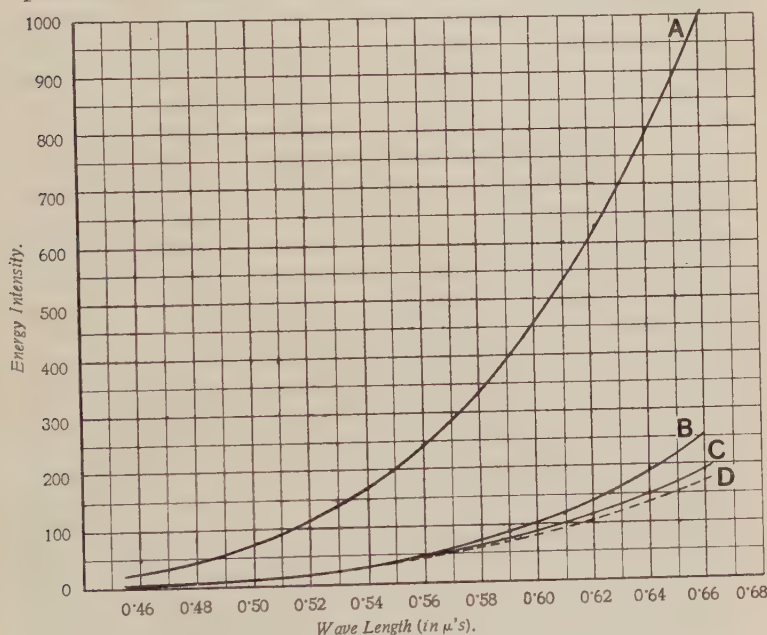
FIG. 5.

Energy intensity curves in the visible spectrum for :—
  A. Black body at 1,750°C.
  B.   ,,   ,, at 1,578°C.
  C. A grey ,, at 1,750°C.
  D. Platinum under open radiation conditions plotted from values given by Waidner and Burgess.

meter, in the green ($\lambda=0.547\mu$) it will be given a value of 1,578°C. This particular grey body curve has been taken in order that it may be comparable with Waidner and Burgess's platinum curve, in which the black body temperature of the platinum melting point at $\lambda=0.547\mu$, is given as 1,578°C. Curve B is the true black body curve for a temperature of

* Hyde, " Journ." Franklin Inst., *loc. cit.*, shows curves very similar to these to illustrate points connected with his investigations on selectivity.

1.578° C., and has been drawn in order to show the difference between the black body and grey body power curves referred to the same intensity of radiation in the green.

The ordinates are relatively so small at the shorter wave-lengths that the crossing of curves B and C at $\lambda = 0.547\mu$ cannot be distinguished. They do, however, actually cross. It is well to notice from curves B and C what a difference in light distribution at different wave lengths exists between the black body and grey body at the same apparent temperature (measured optically). It is this difference of relative distribution which results, by the colour identity method, in the grey body (curve C) being given a temperature of 1.750 C. and the black body (curve B) a temperature of 1.578 C.

Now, the curve for platinum at its melting point lies close to curve C for a grey body. Waidner and Burgess's platinum melting point determinations for the three wave-lengths given in Table VI. are plotted in curve D. If the figures published by Waidner and Burgess may be depended on to give the relative intensities in the three wave-lengths, curve D indicates that the colour identity method should have given a value for the melting point of platinum above 1.750 C., since, as compared with the grey body, Waidner and Burgess show relatively more radiation from platinum at the blue than at the red end of the spectrum. Before definite conclusions can be drawn it would be desirable to have measurements in other than the three wave-lengths considered and information as to the monochromatism of the light in each of the wave-lengths for which the intensities are plotted in curve D. The figures for the melting point of platinum, shown in Table V., indicate, it is true, a tendency to fall in the direction to be expected from Waidner and Burgess's values plotted in the diagram, but not, however, as much as line D indicates. The opinion, therefore, expressed by Hyde that the colour identity method will err in ascribing temperatures which are, if anything, slightly too high, is supported so long as the bodies in question are selective *in the visible spectrum* in favour of the shorter wave-lengths. If they are selective in favour of the red end, the temperature ascribed will tend to be low whilst if they are true grey bodies the method will be accurate.

It should, however, be recalled that compared with other substances platinum is regarded as a relatively selective body in the visible spectrum, and if this is so, the differences indicated in Fig. 6 are for a fairly extreme case,

Little is actually known as yet regarding the departure of metallic bodies from the characteristics of grey bodies in the *region of the visible spectrum.* Any deviation which there is would seem to be small in amount and insufficient to invalidate estimations of the temperature by the colour identity method, intelligently used. It is suggested that the method should be specially useful for assisting in the determination of temperatures and melting points of some of the more refractory substances whose true temperatures by the usual optical methods and assumptions are admittedly open to doubt.

It is of interest now to see what are the temperatures of filaments in gas-filled lamps determined by the colour identity method of measurement.*

In estimating the temperature of the filament in a lamp bulb containing gas it must be remembered that the relation between lumens/watt and temperature cannot be the same as in the ordinary vacuum lamp because of the considerable number of watts carried away from the filament by convection in the gas. Further, the proportion of watts convected depends considerably on the diameter of the wire and on the density of the gas in the bulb.

Six gas-filled lamps have been compared against an ordinary vacuum tungsten lamp in order to determine the difference of efficiency expressed in watts per mean spherical candle or lumens per watt between the two types of lamps when the colour of their radiations is identical, and therefore, when, to a close approximation their temperatures are the same.

Comparisons were made up to an efficiency of about 0·75 watts/mean spherical candle for the vacuum lamp, or about 0·9 watts/mean spherical candle for the gas-filled lamp. The results are shown in Fig. 5, where watts/mean spherical candle for the vacuum lamp is plotted as ordinate and watts/mean spherical candle for the gas-filled lamp as abscissa. The considerable difference between these curves must be ascribed to differences in the amount of gas in the bulbs and to varying diameters of filaments and spirals.

The curves have been extrapolated to pass through the zero of the diagram, and it will be seen that all the points lie on a straight line which passes through the origin except at the comparatively low values of efficiency.

* *See* Langmuir, " Proc." Amer. Inst. of Elect, Eng., Vol. XXXII., p. 1895.

The ordinary working efficiency of gas-filled lamps is at the present time about 0·7 watts mean spherical candle, corresponding in identity of colour of radiation with the vacuum lamp at 0·5 watts mean spherical candle — *i.e.*, 25 lumens per watt approximately.

From the equation to the curve shown on Fig. 3 this gives a temperature of 2,800°C. abs. for the ordinary working temperature of tungsten in gas-filled lamps.
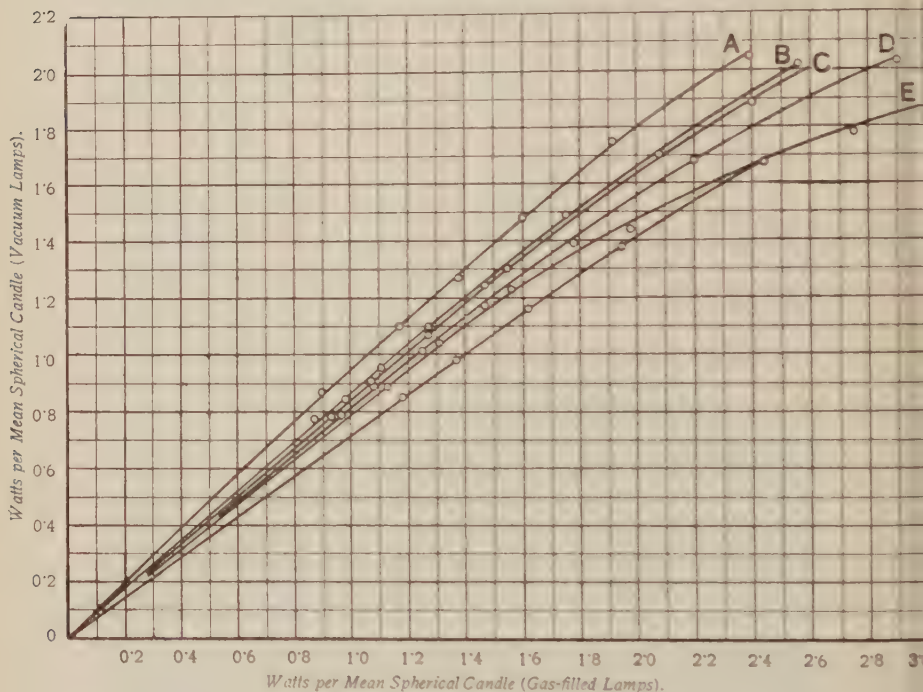
FIG. 6.—GAS-FILLED LAMPS.

Curves showing for various gas-filled tungsten filament lamps A to E, the relation between the actual watts per mean spherical candle and the watts per mean spherical candle of a vacuum lamp filament at the same temperature.

If the half watt lamps are overrun they will be found to burn for a short time satisfactorily at 0·40 watts mean spherical candle, corresponding to a vacuum lamp at 0·32 watts mean spherical candle, or approximately 40 lumens watt. Using the same formula this value of lumens watt corresponds with a temperature of 3,100 C. abs. (2,830 C.). It is clear, then,

that the melting point of tungsten would be above this value, but it cannot be said with certainty how much higher. The comparison lamps used in the determination were of the vacuum type and themselves had tungsten filaments. At 3,000°C. abs. the blackening of the bulbs of such lamps is rapid and liable to lead to error if the temperature of the filament is pushed up to the melting point. The determination of the melting point of tungsten, therefore, by this method could only be undertaken if precautions were taken against errors due to blackening of the bulb. The experiment will be more successful when gas-filled colour standards are made by comparison direct against the black body. Burgess and Le Chatelier* give the melting point of tungsten as $(3,000 \pm 100)$°C., a value with which the authors' observations by the colour identity method are not inconsistent at the highest temperature at which they could safely make measurements.

4. *The Colour of Radiation from the Violle Standard.*

At the beginning of this Paper the importance was explained of giving proper consideration to the colour of the radiation from any proposed standard of light. The increasing efficiencies of modern sources of light owe these increases mainly to higher running temperatures, and hence the light emitted tends to consist of a relatively larger proportion of shorter waves. The light from the Hefner lamp has been found by the authors to correspond in colour with that from a black body at 1,540°C. That from the Pentane lamp to 1,610°C.

The Violle standard consisting as it does of platinum at the melting point (1,750°C.) has a radiation which corresponds with the colour of a carbon filament glow lamp running at an efficiency of $2 \cdot 6_5$ lumens per watt, or $4 \cdot 7_5$ watts per mean spherical candle (*see* Fig. 3). Assuming a reduction factor of $0 \cdot 85$ this is equivalent to an ordinary carbon filament lamp with a specific consumption of $3 \cdot 8$ watts per mean horizontal candle.

The authors wish to express their acknowledgment in connection with this work to Dr. R. T. Glazebrook, C.B., F.R.S., Director of the National Physical Laboratory, and also to Dr. J. A. Harker, F.R.S., for facilities afforded in the use of the black body furnace referred to at the beginning of the Paper.

* *Loc. cit.,* p. 492.

DISCUSSION.

Prof. S. P. THOMPSON thought something must be wrong with the English language when " white " light had to be defined as that radiated by a "black" body. The term "grey" body seemed indefinite. There were many shades of grey, and it would be interesting to know how many bodies were really " grey " according to the precise definition of the authors. He thought that in plotting lumens per watt against temperature it would have accorded better with custom to have plotted temperatures as abscissæ instead of as ordinates.

Mr. A. P. TROTTER (communicated remarks): If this Paper had preceded the introduction of the optical pyrometer, that instrument would not perhaps have gained such a footing. Notwithstanding the apparent advantage that the intensity of light for any given wave-length varied as the fifth power of the temperature, while that of the whole light varied as the fourth power, the principle of colour identity seemed clearly to be the right one for measuring high temperatures, and the use of an arbitrary coloured screen, the wrong one. The estimation of high temperatures by observing the colour of an incandescent body was perhaps practised by Tubal Cain in judging forging and welding heat. What the blacksmith calls " just red at the back of the forge " is about 400°C., and " cherry red " is about 1,650°C., but it has remained for the authors to convert it from a matter of judgment to one of accurate measurement. The method directs attention to small colour differences which in ordinary photometry are deliberately and sometimes with difficulty ignored. This difficulty drives some observers to the flicker photometer. The difficulty is reduced by experience. Possibly some eyes are more sensitive to differences of brilliance and others to differences of hue. The authors are justified in the use of the expression " grey body." The ideal black body which absorbs all radiations and reflects none, and for any temperature radiates more than any other body, is rather repulsive to some people, for it is inseparably connected in their minds with the complicated laws of Stefan, Planck and Wien. The opposite of this would be a body which absorbs no radiations, is a perfect reflector, does not emit any radiation when at a high temperature and entails no mathematics. No such body is known. Intermediate bodies are of two classes, and in their radiant properties they may be called selective and grey, just as bodies between the extreme hues of black and white are coloured and grey. There is an indefinitely large number, several thousands, of perceptibly different shades of grey, and the degree of greyness of a radiant body is merely the factor by which the ordinates of such a curve as C, Fig. 5, must be multiplied to fit curve A. If it does not fit, but cuts it, it is not grey but selective. The authors do not seem to be justified in stating that white light means light of any colour emitted by a black body. It is generally recognised that there can be no definite standard of white light, but there are many substances such as magnesia, snow, or even paper, which are white, and white light is best represented by sunlight reflected from such a substance. But the convention is that it must be our sun, not the hotter Sirius, or the cooler Antares. A grey body also reflects pure white light, but not so much as a white body. In colorimetric tests as made in chemical and physiological laboratories it is found that two tints may appear to one observer to be of identical hue, while to another they differ. This occurs when the two hues differ spectroscopically, and when the eyes of the observers differ in colour sensitivity. In the present method of colour identity, so long as the hues are in general of similar spectroscopic character this difficulty would not arise. When a piece of white porcelain with a dark pattern is heated red hot, the pattern becomes brighter than the background. An optical pyrometer would indicate that the dark pattern was at a higher temperature. But the author's method would reduce the brilliance of the pattern to that of the ground, would show that the hues are identical, and would indicate equality of temperature. The weak point of the ordinary optical pyrometer with a screen as monochromatic as is practicable is that it is applicable only to bodies which approxi-

mate to the conditions of the ideal black body. It is interesting that so useful a side issue should have arisen from the investigation of the Violle standard of light which seems less likely to become a practical one since the publication of Dr. Petavel's work than it did before.

Dr. J. A. HARKER said that to one accustomed to look at the physics of optical pyrometry in the ordinary way it is difficult at first sight to see why platinum at its melting point should emit the same colour of radiation as a black body at the same temperature. In fact one's predisposition would be to the opinion that this is quite unlikely. The accuracy with which the authors by their method determined the value for the melting point of platinum was very surprising, and the considerations the authors bring forward put a new complexion on one's conceptions. With regard to the "black body" used by the authors, it was extremely difficult to obtain a furnace at these high temperatures without a cloudy atmosphere, but after distilling the impurities out of the carbon—which was the only suitable substance to use—he had found it possible to obtain a high temperature furnace with a perfectly clear atmosphere. With the ordinary optical pyrometer it was impossible by means of the coloured glass supplied to get sufficiently perfect monochromatism to give great accuracy. If a strip of platinum be used instead of a wire it is possible to maintain it within a degree or two of its melting point for some time.

Mr. A. CAMPBELL asked how the optical pyrometer was calibrated at high temperatures. In the case of the Violle standard, was it essential actually to melt the standard strip ? Would it not be more satisfactory to use a tungsten strip heated up until a small speck of (e.g.), quartz on it began to melt rather than to melt the strip which was under observation ?

Mr. J. S. Dow doubted if metal filament lamps could be regarded as strictly grey through their visible spectrum. He recalled a series of articles by W. Coblentz in the "Illuminating Engineer," in 1910, showing how metals in general had a low emissivity in the infra-red, and this suggests irregularity in the emissivity of a polished metal surface, even in the visible spectrum. The methods the authors proposed seemed to require less manipulative skill than most pyrometers. For example, one observer had discovered an interference effect that might give rise to considerable errors in the case of instruments involving the inspection of a bright filament against a luminous background. Measurements with a photometer would be free from this source of error. The authors had found that half-watt lamps could be run for a short time at 0·4 watt per mean spherical candle. Could they, by extrapolation, state the approximate limiting theoretical efficiency of a tungsten filament ? Dr. Lux had recently estimated that the melting point of tungsten would be approached at 0·3 watt per candle.

Mr. A. W. BEUTTELL suggested that any difference in the colour sensations of the observer from the normal would affect the results obtained by the method.

Dr. C. CHREE asked which of the values, 1,750°C. or 1,770°C., the authors considered most nearly correct.

Mr. E. H. RAYNER thought it might add to the sensitiveness of the method if, when the colour match had been obtained, coloured glasses—say, first a red and then a blue glass—were put in front of the eye. Any inexactness in the match might be increased and shown up in this way.

Mr. J. GUILD (communicated remarks) : The auxiliary adjustment for equality of brightness mentioned by the authors on page 236 is an important one, inasmuch as, on account of the change in the sensitivity curve of the eye with change in brightness of the incident light, it is only when both sources produce the same illumination *at the eye* that identity in the colour perceived involves identity in their energy distribution curves over the visible spectrum. If, for example, one adjusted two sources to give identity of hue but neglected to equalise the brightness at the photometer the source producing the weaker light would require to be at a lower temperature, *i.e.*, to have more red in its spectrum than the other, since, for the weaker light,

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the red-sensitivity of the eye bears a smaller ratio to the total sensitivity than for the stronger. In what way does the accuracy of the colour-identity method vary with temperature? To the casual observer the colour of a furnace appears to vary much more rapidly with temperature at low red and orange heats than at higher temperatures, and one would expect considerable precision at, say, 700°C. or 800°C., and a progressive falling off of sensitiveness as the temperature is increased. This could be calculated from the radiation laws and the chromatic properties of the eye, but a series of test experiments over a wide temperature range would be more convincing.

Mr. PATERSON thanked the speakers for their remarks. In answer to Prof. Thompson, Mr. Trotter's contention regarding " grey " bodies seems to explain the matter clearly. The convention of restricting the term " grey " body to one which radiated in the way shown in curve C, Fig. 5, is a very usual one. There could be an indefinite number of such curves, but each had the property that some one multiplier would make all its ordinates coincide with a black body curve at the same true temperature as the grey bodies in question. A body whose curve would not conform with this requirement would be spoken of as a "selective" body rather than as a " grey " body. The question is mainly one of nomenclature and definition. In the American literature the above convention is generally accepted. According to Prof. Thompson and Mr. Trotter the authors may be wrong in suggesting that white light is the light emitted by a black body at any temperature. Mr. Trotter prefers to restrict it to light radiated by the sun. He was still of opinion that " white light " was a good term to use, since it conveys the impression of a mixture of all wave-lengths in definite proportions. If " white light " were to be regarded as the light emitted by the sun, it would be very indefinite owing to atmospheric absorption.* In reply to Mr. Campbell, no effort has been made by the authors to use platinum strip as a standard of light, but experiments had been made by others on these lines without much success. In reply to Mr. Dow, several experimenters had come to the conclusion from the evidence at their disposal that these materials were mainly selective in favour of the visible spectrum as a whole. The authors had now shown experimentally that platinum had the same relative distribution of light as a black body at the same temperature, and this appears to the authors important evidence to justify the last statement, that is to say, that they approximate to " grey " bodies in the visible spectrum. The authors had compared a tungsten filament with a carbon filament over the visible spectrum with a spectrophotometer, and to the accuracy to which the instrument could be used no difference in selectivity could be detected. Over the infra-red portion of the spectrum it is well known that a large difference exists. An answer to the question regarding the limiting efficiency of half-watt lamps is given in the Paper. In reply to Mr. Bentell, a person with defective colour sight should only be less sensitive than one with normal vision, but on the average should not obtain different results. In reply to Dr. Chree, the melting point of platinum is usually given as 1,750°C. + or - 20°C. The present experiments were carried out not to determine the melting point of platinum but to show from the melting point values obtained the accuracy of the method of colour identity. Mr. Rayner's suggestion is certainly worth trying and ought to yield very interesting results. In reply to Mr. Guild, it is impossible to obtain identity of colour with accuracy unless illuminations at the two sides of the photometer are the same. There is a slight difference in sensitiveness of the colour identity method at different temperatures, for instance, a definite change of colour which is caused by 1 per cent. in temperature at 2,000 deg. abs. is caused by 1·6 per cent. at 2,800 deg. abs. Regarding Dr. Harker's remarks, the authors hope to take advantage of the offer made by him to use one of his black body furnaces up to higher temperatures. In doing so they would add to the obligation they are already under to Dr. Harker in connection with the furnaces used in the present work.

* See " Modern Illuminants." Gaster and Dow. P. 181.

XX.—*The Unit of Candle-power in White Light.** By* CLIFFORD C. PATERSON *and* B. P. DUDDING, *A.R.C.Sc.* (*From the National Physical Laboratory.*)

Synopsis.

1. The differences in the colour of the light radiated from different sources of white light causes uncertainties in photometric determinations. The Paper describes the methods used at the National Physical Laboratory to minimise this well-known difficulty by the use of the cascade principle.

2. The six sets of electric substandards of candle-power are described, which vary in the colour of the light radiated, from that of the pentane lamp (red) to that of a tungsten vacuum lamp operating at 1·5 watts per candle.

3. The differences between the values obtained by different observers in the process of stepping from one colour to the next are discussed and compared with those obtained by the direct comparison of the first and last sets of substandards.

4. The probable errors of the determination are discussed and shown to be of the order of $0·2_5$ per cent.

5. The absolute value of the unit of candle-power has been re-determined from the pentane lamp, and found to agree with the determination made by one of the authors 10 years ago to within less than 0·1 per cent.

6. The corrections to the pentane lamp for humidity and barometric changes were also redetermined, and found to agree within narrow limits with those previously found by one of the authors.

7. The relation between the humidity and temperature corrections for the pentane lamp is discussed, and the fact that these two effects may act together is suggested as the reason for a discrepancy which has been noticed between the humidity constants of the lamp as determined in London and Washington.

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* The work dealt with in the various sections of this Paper has entailed so many thousands of measurements and candle-power determinations of individual lamps, that to give them in detail would greatly overweight the Paper with tabular matter. The authors have, therefore, felt obliged to limit themselves to summaries of results and sometimes to bare statements of the results obtained, computed from their manuscript tables.

General Discussion.

The principal object of this Paper is to describe and discuss the methods which have been used at the National Physical Laboratory, and the results obtained in the standardisation of high efficiency metal filament lamps in terms of the unit given by the 10-candle pentane lamp. Such a standardisation entails the comparison of two sources whose radiation in the visible spectrum, though continuous, differs appreciably in hue. The amount of this difference may be gauged by remembering that if the voltage on an ordinary carbon filament lamp is lowered until the hue of its light matches that from the pentane lamp, the carbon lamp will be found to be burning at a rate of consumption of about 7·7 watts per mean horizontal candle. The ordinary consumption of carbon filament lamps is in the neighbourhood of 4 watts per mean horizontal candle, and of tungsten lamps between 1·0 and 1·5 watts per mean horizontal candle. If the Hefner lamp were matched on the same basis the specific consumption would be still higher, viz., at the rate of about 11·0 watts per candle.

In approaching this question some criterion is wanted in terms of which differences in the hue of light may be expressed. In this Paper, spectral colours are not being dealt with, but only so called white light—that is to say, light whose spectral distribution is continuous and not very different from that radiated by a black or grey body. The most common way of defining the hue of white light is to state the specific consumption in watts per candle of an incandescent lamp, the hue of whose radiation is identical with it. This value of specific consumption, however, will not be the same for a carbon as for a tungsten lamp, both of whose radiations have the same hue. A reason for the difference is the tendency for the tungsten lamp to radiate selectively in favour of those wave lengths lying within the visible spectrum. It is necessary, therefore, if this basis of comparison is used, to say if the watts per candle are on the carbon or tungsten basis, and hence it will be seen that although the values of watts per candle of incandescent lamps are convenient as a general guide to colour, this basis of measurement is arbitrary, and not very satisfactory for precise definition.

A series of comparisons was therefore carried out in order to express the hue of radiation from electric glow lamps in terms of the temperature of a black body whose radiation was identical in colour with that of the glow lamps in question. It
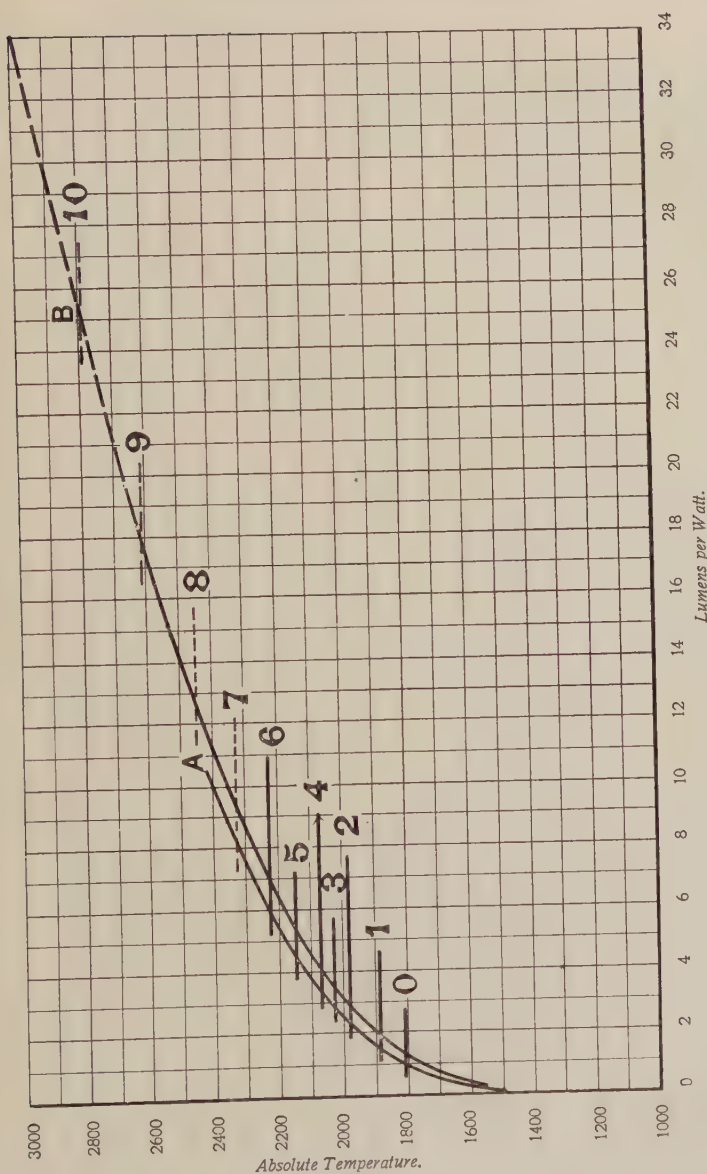
Fig. 1.—Ordinary Tungsten (Curve B) and Carbon (Curve A) Filament Vacuum Lamps. Curves connecting lumens per watt* of the lamps with the temperature of a black body whose radiation is identical in colour with that of the lamps.

* Assuming a ratio of $\dfrac{\text{M.S.C.P.}}{\text{M.H.C.P.}}$ of 0·85 and 0·79 for carbon and tungsten filaments respectively, the approximate watts per candle are obtained for any value of lumens per watt by dividing the latter into 10·7 for carbon filament lamps, and 10·0 for tungsten filament lamps.

has long been recognised that such colour comparisons can be made very accurately with a Lummer Brodhun photometer.* The methods used are those described in another Paper by the authors,† which should be referred to for further details.

In Fig. 1 are reproduced curves from this Paper in which the ordinates are temperatures and lumens per watt abscissæ. Curve A shows the lumens per watt of a carbon lamp corresponding with the temperature of a black body whose radiation is identical in colour with that from the lamp. Curve B gives the same relationship for tungsten extrapolated according to the equation found to express this relationship, the extrapolated portion of the curve being shown by a dotted line. Temperatures are on the optical scale, using a dominant wavelength of $\lambda = 0.650\mu$ giving the melting point of platinum as about 1,750°C.

From this diagram (line 1) it will be seen that a carbon lamp, the hue of whose radiation matches that from the pentane lamp, has the same coloured radiation as a black body at 1,610°C., whilst a tungsten lamp at 1·5 watts per mean horizontal candle (line 6) matches a black body at 1,950°C. Now, between these extremes there exists a considerable colour difference, and this has to be bridged photometrically when defining the candlepower of the tungsten lamp in terms of the pentane unit. If, in order to certify them, tungsten filament sub-standards were compared on every occasion against the pentane lamp, or substandards of the same colour, the large colour difference would be a constant source of trouble and uncertainty. To obviate this difficulty, a set of metal filament sub-standards running at 1·5 watts per mean horizontal candle has been standardised, by the cascade method and values of candle-power in terms of the pentane unit have been assigned to them once for all. The method used and results obtained in standardising these tungsten lamps are the principal subjects of this Paper.

The Cascade Method.

Briefly, the course followed has been to interpose between the pentane lamp and the high-efficiency sub-standards four

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* Hyde, Cady and Middlekauff, " Selective Emission of Incandescent Lamps." Ill. Eng. Soc., New York. Vol. IV., 1909.
† Paterson and Dudding, " The Estimation of High Temperatures by the Method of Colour Identity." " Proc." Phys. Soc., London. Vol. XXVII., p. 230.

additional sets of lamps, each varying from the next in effi-
ciency (or operating temperature) by such an amount that the
difference in the hue of their radiations constitutes a regular
colour gradation in approximately even steps from the pentane
colour up to that of the 1·5 watts per candle tungsten filament
standards.   These steps are marked respectively by the hori-
zontal lines 2, 3, 4 and 5 on Fig. 1.*   If, now, each such set of
sub-standards is standardised against the set below it—that is
to say, the set whose light is one stage redder than its own—
it is possible to arrive at a value for the final set by a series
of photometric comparisons, in each of which the colour
difference, although perceptible, is too small seriously to
trouble an observer with normal sight.

It should be said at once that this system does not eliminate
the personal error in colour photometry.   It merely divides a
large colour step into a number of small ones.   It is known,
however, that with scarcely perceptible colour differences
the photometric agreement between different observers with
normal sight is exceedingly close and the measurements can be
made to a high accuracy.   On the other hand, photometry
with large colour differences, although possible in commercial
work, gives rise to erratic and inconsistent readings when
judged from the standpoint of the higher precision required
for standardisation.

It is assumed in what follows that in the measurements of
lights of different colours the true basis of comparison is the
perception of the average normal eye.   It is for the use of the
human eye that artificial illumination is chiefly required, and
it is suggested that two lights should be considered equally
intense when the illuminations produced by them appear to be
equal to a person with average normal sight.

The nearest approximation which is was possible to make to
normal sight was to take the average of six different observers.
Each one of these carried out the complete series of com-
parisons, and none of them had abnormal colour vision.   The
results of the investigation show that only very small errors
can be introduced by assuming that the mean result will
represent the average of a much larger number of persons.

The photometric work involved has been laborious.   In
order to eliminate any uncertainties of individual lamps and to

* Set 3 will be found to differ from its neighbours by smaller amounts than
the other sets.   It was included in the series because it corresponded in
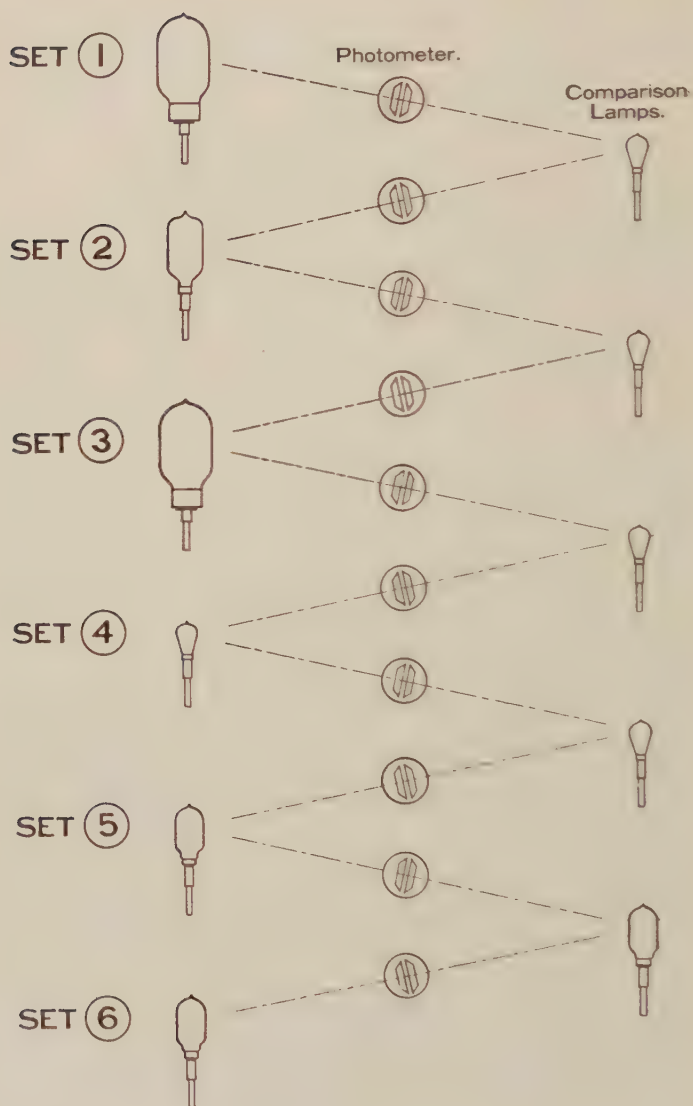colour with the ordinary carbon filament standard lamps.

Fig. 2.—Diagram showing scheme of "Cascade" comparison used in the realisation of the unit of candle-power in lamps operating at 1·5 watts per mean horizontal candle (set 6).

reduce the probable error, each of the six sets of lamps consisted of 10 or 12 individual lamps, and each set was compared with the next one, in order of colour, some six or eight times by each observer. This entailed in all between 2,000 and 3,000 standardisations, or between 10,000 and 15,000 observations. The work was started several years ago, but the difficulty experienced at first in obtaining metal filament standards which were sufficiently constant caused much delay in the early stages.

Fig. 2 indicates graphically the scheme of the cascade comparison, and the relation of each group of standards to the remainder.

Set 1 represents a series of 11 sub-standards whose radiation matches in hue that of the pentane lamp, or the radiation from a black body at 1,610°C. Set 2 indicates a series of 11 Fleming-Ediswan sub-standards operating at an equivalent temperature of 1,700°C. Set 2 was compared against set 1 by the usual substitution method of photometry, using a Lummer-Brodhun photometer, and a comparison lamp, the colour of whose radiation lay midway between that of sets 1 and 2. In a similar way, set 3, with an equivalent temperature of about 1,750°C., was compared against 2 through another comparison lamp, which also divided the colour difference, and so on to set 6. It will thus be seen that by such a substitution method as this the colour difference between sets 1 and 6 is divided into 10 approximately equal steps—that is to say, at any one measurement there is only a colour difference of one-tenth that of the difference between sets 1 and 6. The difference of colour in the photometer throughout the investigation was, therefore, never greater than that resulting from a difference of temperature of about 45°C. All the measurements were made with an illumination at the photometer screen of 10 metre-candles. Finally, having by this means, and for each observer, assigned candle-power values to each of the lamps in set 6, the latter were compared in one step with set 1.

It is of interest to note that the colour of the radiation from melting platinum is the same as that of set 3, marked by line 3 in Fig. 1.*

Before passing to the details of the work the authors desire to point out one of the advantages of the cascade system as

* *Loc. cit.*

applied to electric sub-standards. The possession at a stan-
dardising laboratory of a regular gradation of sub-standards
ranging from the lowest to the highest efficiencies. enables a
set of standards of suitable colour to be chosen for whatever
test has to be undertaken. As gas-filled lamps develop and
come more into use there would seem to be no reason why the
system should not be extended to very high efficiencies with
sub-standards eventually covering a range of from 1.000°C.
to about 2,700°C. The authors are at present experimenting
with gas-filled standard lamps with the intention of carrying
the work a stage further should they prove satisfactory.

It is found that four more steps are required to connect up the
existing set 6, with the standards required for the testing of
present-day gas-filled lamps. These are indicated by hori-
zontal dotted lines in Fig. 1, marked 7, 8, 9 and 10.

Lamps Used in the Investigation.

The types of lamps used in the investigation are shown in
Fig. 3. Some of them. such as the Fleming-Ediswan lamps
(set 2) date back from 1902, and are the type of carbon filament
standards due to Dr. J. A. Fleming, F.R.S.. which have been
used at the Laboratory since then for maintaining the unit of
candle-power. Set 4 are " gem " lamps with so-called metal-
lised carbon filaments, and were the best lamps which could be
found at the time they were installed. for giving constant results
at approximately 3·5 watts per mean horizontal candle. The
remaining lamps have tungsten filaments and have been
specially made for the Laboratory by the Osram Lamp Works
and presented at different times by the General Electric Co.
Reliable tungsten standards which can be depended on for
constancy when operated at high efficiencies are not easy to
obtain. Many tungsten lamps are liable to small erratic
fluctuations of candle-power. the reason for which is obscure.
Such fluctuations being small, do not affect the lamps for
ordinary lighting purposes, but are a serious drawback for
standard work. Each set of lamps. therefore. required careful
preliminary testing. and many had to be discarded in the pro-
cess. In the early days of the tungsten lamp several complete
sets of standards had to be discarded after their seasoning was
finished and standardisation work had begun. due to the
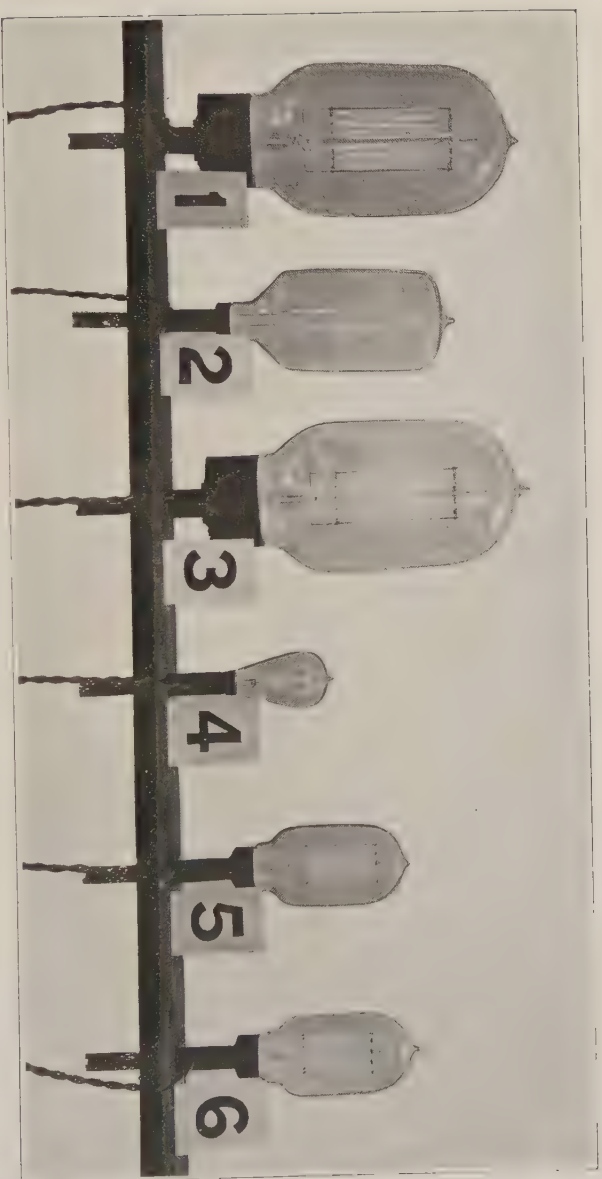appearance of small changes of candle-power.

Fig. 3.—Representative Lamps from each Set of Sub-standards.

The details of the various sets of lamps are given in Table I.

TABLE I.

Set.	Approx. watts per M.H.C.	No. of lamps.	Temp. of black body for colour match.	Approx. c.p.	Description.
1	7·7 (Carbon basis)	11	1,610°C.	15	Six tungsten hairpin filaments suspended in grid form in one plane. Height of filaments 120 mm. Volts 105. Matches the colour of the 10 candle pentane lamp.
2	4·8 (Carbon basis)	11	1,700°C.	15	Single carbon loop filament in one plane. Height of filament 110 mm. Volts 100.
3	3·9 (Carbon basis)	10	1,750°C.	32	Five tungsten hairpin filaments suspended in grid form in one plane. Height of filaments 120 mm. Volts 109.
4	3·3 (Carbon basis)	10	1,790°C.	15	Gem coiled metallised carbon filaments (usual pattern). Volts 100.
5	1·9 (Tungsten basis)	13	1,870°C.	12	Four tungsten hairpin filaments suspended in grid form in one plane. Height of filaments 70 mm. Volts 95.
6	1·5 (Tungsten basis)	10	1,950°C.	18	Four tungsten hairpin filaments suspended in grid form in one plane. Height of filaments 70 mm. Volts 105.

Absolute Value of the Unit of Candle-power.

The foregoing description deals with the method used to determine the ratio of the average value of one set of sub-standards in terms of another, and, therefore, deals only with relative values. The *absolute* values of candle-power of sets 1 and 2 have been assigned to them respectively by two independent comparisons with the Harcourt 10 candle standard lamp. Set No. 2 (consisting of Fleming-Ediswan lamps) was compared against the pentane lamp in 1904-6. It will be seen from Table I. that the light from set 2 was somewhat whiter than that from the pentane lamp. At the time, however, that the determination was made their colour formed a suitable intermediate step between that of the pentane lamp and the usual light sources which had to be tested. After eight years' use of these lamps, and in view of some small unexplained differences between the N.P.L. unit and that determined in other quarters, it was considered desirable to realise the unit anew. Set No. 1 of the sub-standards was, therefore, prepared, whose light was identical in colour with that from the pentane lamp. These lamps were compared against the pentane standard in the usual way over a period of about two years (1912-14). The

results were analysed by the method of least squares in order to determine the true value of the sub-standards when the pentane lamp was burning under standard atmospheric conditions. It is satisfactory to find from the subsequent intercomparison of sets 1 and 2 that the unit of candle-power realised on these two different occasions separated by a period of eight years agreed with each other to within less than 1 part in 1,000 ($0 \cdot _{07}$ per cent.). The value of the unit originally realised in set 2 is that which formed the subject of agreement between the laboratories of this country, France and the United States in 1908. The value obtained in the original determination has, therefore, now been fully substantiated by the new determination, and set 1 of the sub-standards becomes a fundamental set whose constancy is well assured, and the determination of whose candle-power values is independent of any colour difference. Incidentally, it has been possible to redetermine the constants of the pentane lamp affecting its changes with varying atmospheric conditions. These results are discussed in detail later.

It must be admitted that it is to some extent a matter of good fortune that the two independent determinations of the unit have agreed to within such close limits, since the first determination was made by one observer only, and since there existed a small colour difference his readings might well have differed from the average by a few parts in 1,000. It so happens, however, that his difference from the mean was in this instance negligible, and the magnitude of the unit as determined 10 years ago remains unchanged.

The following points of general interest arising from these and allied measurements will now be discussed.

1. The order of agreement between different observers in the final candle-power value obtained for the high efficiency lamps (set 6).

> (*a*) By the cascade method ;
> (*b*) By comparison in one step.

2. The accuracy to which photometric readings of this description are repeated by any observer and the probable errors of the results.

3. The order of agreement with other National Laboratories in the values assigned to high efficiency glow lamps which

differ in the hue of their radiation from the flame standards from which the fundamental units are derived.

4. The constants of the pentane lamp with changes of humidity, barometer and temperature.

5. The constancy of the unit of candle-power maintained through the medium of electric sub-standards.

1. *Values of Candle-power Obtained for the High Efficiency Tungsten Lamps.*

The precise numerical values obtained for the individual lamps are not of immediate interest. For the purpose of this Paper each set of lamps may be considered as one lamp. If there was evidence in the course of the work that any one lamp was undergoing a progressive change it was discarded, so that the figures given may be assumed in each case to be those for a set of lamps whose candle-powers have remained unchanged during the comparison.

Throughout the work the average of all six observers has been taken as giving the true value for any set of lamps. For the purpose, therefore, of showing the differences of individual observers this average value is taken as 100 for each of the six sets of lamps measured by the cascade method. The observers are designated by the letters A, B, C, D, E, and F.

Table II. gives a summary of the results. Observer A compared set 2 against set 1 about eight times, and obtained a value of 99.5_5 for set 2 as compared with a mean value for all observers, of 100. He then compared 3 against 2, assuming *his own* value of 99.5_5 for set 2. In the same way 4 is compared against 3 and so on to set 6. The bottom line, therefore, gives the values which each observer obtains for set 6 in terms of set 1 as a result of his own independent descent through the intermediate steps of the cascade.

TABLE II.—*Summary of Cascade Observations, each Set being compared by each Observer against the one immediately above it.*

Observer.	A.	B.	C.	D.	E.	F.	Average.
Set 1.........	100·00	100·00	100·00	100·00	100·00	100·00	100
,, 2.........	99.5_5	99.4_5	99.9_0	99.8_7	100.8_4	100.3_9	100
,, 3.........	99.4_3	99.3_8	99.9_0	99.9_6	100.8_8	100.4_5	100
,, 4.........	98.9_5	99.0_4	99.8_4	100.3_1	101.3_6	100.5_0	100
,, 5.........	98.7_3	98.7_0	99.7_9	100.4_5	101.4_6	100.8_7	100
,, 6.........	98.5_4	98.6_1	99.6_9	100.7_1	101.4_8	100.9_7	100

Table III. gives the value which each observer obtains for set 6 when compared in *one* step against set 1 with the Lummer

Brodhun photometer. It will be seen in the last column that this direct comparison of set 6 with set 1, entailing the full colour difference, yielded as the mean of all observers a result differing by a little less than 0·3 per cent. from the comparison by the cascade method (Table II.). By reference to Table V. it will be seen that this is the order of the probable error of either result.

TABLE III.—*Summary of Direct Comparison of Set 6 against Set 1 by each Observer.*

Observer.	A.	B.	C.	D.	E.	F.	Average.
Set 6.........	99·0₄	99·5₅	98·9₃	99·3·	100·9₂	100·4₁	99·7₂

Judged from the physiological standpoint, it cannot be said with certainty that the cascade method (Table II.) should give a result which is more absolutely rigorous than the direct method (Table III.). The case is however, somewhat analogous to the comparison of a low with a high resistance direct, and through the medium of a series of resistances with intermediate values. The authors place more reliance on the cascade method because of the less consistent manner in which observers by the direct method have been found to repeat their individual readings over considerable intervals of time. This lack of consistency has not been found where the small colour differences of the cascade method are concerned. All the measurements with the direct step were taken over a relatively short period of time whilst those included in the cascade method extended over several years. The experience of the authors where larger colour intervals are concerned does not afford an assurance that the "day to day" probable error of the direct step would have been so small, had the measurements in this case also extended over a longer period.

It will be seen that the largest difference of any observer from the mean in the first result by the cascade method was about 1·5 per cent. in the case of observer E. His mean difference per step was, therefore, about 0·3 per cent. By the direct method in one step the greatest difference was again by observer E, viz., 1·2₄ per cent.

An examination of the differences from the mean result revealed by each observer for each successive step in the cascade is facilitated by Table IV.

TABLE IV.—*Showing how much for each Step in the Cascade each Observer differed from the Mean of all for that Step.*

Comparisons.	Observer.					
	A.	B.	C.	D.	E.	F.
	$\%$	$\%$	$\%$	$\%$	$\%$	$\%$
Set 2 against Set 1	-0.4_5	-0.5_5	-0.1_0	-0.1_8	$+0.8_4$	$+0.3_9$
,, 3 ,, ,, 2	-0.1_2	-0.0_7	-0.0_0	$+0.0_9$	$+0.0_4$	$+0.0_6$
,, 4 ,, ,, 3	-0.4_8	-0.3_4	-0.0_6	$+0.3_5$	$+0.4_8$	$+0.0_5$
,, 5 ,, ,, 4	-0.2_2	-0.3_4	-0.0_5	$+0.1_4$	$+0.1_0$	$+0.3_7$
,, 6 ,, ,, 5	-0.1_9	-0.0_9	-0.1_0	$+0.2_6$	$+0.0_2$	$+0.1$
Mean	$-0\cdot$	-0.2_8	-0.0_6	$+0.1_4$	$+0.3_0$	$+0.1_9$
Total (cascade)	-1.4_6	-1.3_9	-0.3_1	$+0.7_1$	$+1.4_8$	$+0.9_7$
Set 6 agnst Set 1(direct)	-0.6	-0.1_7	-0.7_7	-0.3_5	$+1.2_4$	$+0.6_9$

It will be seen from this table that there is, as would be expected, a general tendency for each person's observations to be either higher or lower than the average of all observers throughout the whole series. The amounts by which the determinations of any one observer are high or low for the different steps varied slightly, but no more than would be expected from Table V., showing the probable errors of the comparisons.

Observer E obtained rather a large difference from the mean, in the comparison of set 2 against set 1, but this appears to be the only slightly anomalous result in all the 30 cascade determinations.

The table shows clearly that even the small colour differences represented by a change of temperature of the filament of about 45°C. give rise to differences, though small, in the personal judgment of the various observers. Furthermore, when any one observer's differences for the five steps of the cascade are added together the total difference does not entirely accord with the results of that observer's *direct* comparison through the large colour step. (Compare the last two lines of the table.) For observers E and F the sum of the differences for the five small steps gave, it is true, nearly the same figure as for the one large step, but in all other cases the differences were appreciable, observers B and D especially showing unexpected results in the direct comparison. This illustrates what was stated earlier as to the more erratic nature of results obtained with large colour steps. The experience obtained with these and other comparisons is that whereas an observer may be relied upon for constancy of judgment in measuring with small colour differences, the same *constancy* of judgment is not obtainable with large ones.

Efforts were made to obtain a comparison between sets 1 and 6 by means of the flicker method. Several flicker photometers were tried by four of the observers, but the precision of the measurements was found to be so inferior to that obtainable with the Lummer Brodhun photometer that after much time had been spent the attempt was abandoned. The authors were unable to experience the sensitivity claimed for the flicker photometer by some other experimenters, and the results they obtained with it were of quite a different order of accuracy from those forming the subject of this Paper. Moreover, it is found that the fatigue occasioned by continuous photometric observation throughout a whole day necessitated by these standardisations was very serious with the flicker photometer, and tended still further to diminish the sensitivity.

2. *Probable Error of the Results.*

In computing the probable error in the value attributed to a lamp by a set of observations, the error may be regarded as arising from three distinct causes.

First, there is the error due mainly to the lack of sensitivity of the photometer, resulting in a succession of readings being obtained which differ slightly amongst themselves—all other variables being constant for the short period during which the lamp is being photometered. Five or more photometric settings have always been made by each observer on each occasion on which the lamp is tested. The usual probable error in the arithmetical mean of these five settings in any step of the cascade is 0·2 per cent. in candle-power, whilst for the direct step with the large colour difference it is 0·3 per cent. The ultimate probable error in the unit of candle-power held in a batch of lamps due to this cause is, however, very small, being reduced approximately in proportion to the square root of the total number of measurements made. This number being very great the error becomes entirely negligible.

The second source of error, called here the "day-to-day error," produces differences in the value obtained for the same lamp as measured from day to day. These discrepancies may be ascribed to the slight differences in the mechanical or electrical adjustments of the lamps, to small changes in the lamps themselves or to variations of the observers' judgment due to physiological or psychological causes. The probable error due to these causes is kept small, by each observer making his measurements on a sufficient number of different days. It

may amount to 0.0_4 per cent. for the determination of the values of one set of lamps in terms of an adjacent one in the cascade series. In computing this error it would not be right to regard the causes as entirely mechanical in character. If they were, the probable error would be much reduced by the fact that there were at least 10 lamps in each set. It is necessary to regard at least a portion of the error as due to physiological causes the effect of which is not reduced by having a number of lamps, but only by increasing the number of observers. In order to avoid too sanguine a figure for the " day-to-day " error, its value has been computed on the assumption that errors under this section are entirely physiological in origin.

The third and most important source of error lies in the consistent differences which exist between various observers, due to colour differences. In the cascade method of bridging the step from the lowest to the highest efficiency standards the errors are additive from one step to the next, and the final probable error has been calculated from the final differences tabulated in line 7 Table IV.

The amount of the probable errors ascribed to each cause for each step of the cascade is given in Table V. It will be seen that the probable error of the final result—viz., of the unit represented by set 6 in terms of set No. 1 is of the order of 0.3_5 per cent. both by the cascade and by the direct methods.

TABLE V.—*Giving the Probable Errors of the Various Steps of the Cascade and Direct Comparisons.*

Comparison.	Probable errors.			Total probable error in mean of set.
	Bench error.	Day-to-day error.	Error due to differences between observers.	
	%	%	%	%
Pentane lamp against set 1	0·01	0·05	0·00	0·05
Set 2 against Set 1	0·01	0.03_5	0·15	0.15_5
,, 2 ,, ,, 2	0·01	0.03_5	0.04_5	0·06
,, 4 ,, ,, 3	0·01	0·04	0·12	0·13
,, 5 ,, ,, 4	0·01	0·03	0·11	0.11_5
,, 6 ,, ,, 5	0·01	0·02	0·04	0.04_5
Total probable error by cascade method Set 6 against Set 1	0·02	0·07	0·36	0·37
Total probable error by direct method, Set 6 against Set 1	0·02	0·06	0.22_5	0·23

3. *Intercomparison with other National Laboratories of the Unit of Candle-power in White Light.*

Having fixed the value of the unit in white light by the cascade method, it became of interest to know what order of agreement existed between it and similar units evolved at the National Laboratories of France, Germany and the United States. A set of 14 tungsten filament sub-standards with a specific consumption of about 1·5 watts per candle was prepared and sent to the Bureau of Standards, Washington. A similar set was prepared and sent to the Physikalisch-Technische Reichsanstalt, Berlin, and the Laboratoire Central d'Electricité, Paris. The lamps were tested both before leaving and after their return to England. At the same time a set of lamps was prepared at the Bureau of Standards and sent over here for test. Ordinary 100-volt metal filament glow lamps were used in these comparisons, and altogether about 18 months elapsed between the initial and the final comparisons. During this period some of the lamps appeared to change somewhat, and differences to the amount of about 0·5 per cent. were observed. In order to eliminate, as far as possible, the effect of these changes, it was assumed that when comparisons between any two laboratories were made over a short period of time no intermediate change in the lamp was likely to have taken place. When, however, the comparisons were spread over a long period of time the N.P.L. value assigned to any lamp was taken as the mean of the initial and final measurements on it.

The following table shows the value of the N.P.L. unit in terms of that of the Reichsanstalt, Bureau of Standards and Laboratoire Central, deduced from the mean of all the lamps tested :—

TABLE VI.

	Ratio.	Difference from accepted value.
Bureau of Standards/N.P.L.	1·00₅	+ 0·₃ %
Reichsanstalt/N.P.L.	0·90₂	0·₃
Laboratoire/N.P.L.	1·00₂	+ 0·₂ %

A higher accuracy than 0·25 per cent. cannot be claimed for these intercomparisons, and the results, therefore, show that within the limits of accuracy of the experiment, the units of candle-power at the four laboratories realised in lamps operat-

ing at 1·5 watts per candle are in virtual agreement. The chief interest of the result lies in the fact that different methods were used at the several laboratories for carrying out the comparison. At the Laboratoire Central the comparison was made by measuring the lamps directly against standards of about 3·5 or 4·0 watts per candle—thus the large colour difference was bridged in one step. The Laboratoire Central values given are based on tests by an observer with normal colour vision. A second observer with abnormal colour vision obtained readings differing by 2 per cent. from these. By the method adopted at the Reichsanstalt a faintly coloured blue glass is placed between the photometer and the lamp of lower efficiency of the two under comparison so as to equalise the colours. This blue glass is calibrated once and for all, and the constants so determined are used in deducing the candle-power of the high efficiency lamps on test.

The values assessed by the Bureau of Standards are the means of comparisons taken (a) directly and (b) by means of a blue glass calibrated photometrically. In this calibration the mean of a number of observers was taken in fixing the trans-mission constants of the glass.

4. *The Redetermination of the Constants of the Pentane Lamp.*

The second realisation of the unit of candle-power from the pentane lamp, referred to on p. 263, has afforded an opportunity of re-examining the constants of the pentane lamp in regard to changes of humidity and barometric pressure. As in the previous determination,* the comparisons extended over two summers and winters, and by this means as wide a range as possible of humidity was secured. The conditions for the use of the lamp were similar to those existing previously, except that a larger number of observers took part in the measure-ments, and there was no colour difference between the light from the pentane lamp and that from the sub-standards com-pared against it. An Assmann ventilated hygrometer was used in the tests, and all humidities are expressed in terms of its indications. In view of the similarity of the experiments to those which have been already described, it is not necessary to give the actual observations in detail here. They have, however, been carefully analysed in the usual way by the method of least squares, and the constants for variation of

* C. C. Paterson, "Investigations on Light Standards," "Journ." I.E.E., Vol. XXXVIII., p. 274.

humidity and for barometric changes are compared below with those previously obtained.

If C.P.=the candle-power of the pentane lamp, $e =$ the humidity in litres of water vapour per cubic metre of moist air, $b=$barometric pressure in millimetres, then

(1904-06 determination) C.P. $= 10\{1+0.0066 \ e - e - 0.0008$
$$(760-b)\} \quad . \quad . \quad (1)$$

(1912-14 determination) C.P. $= 10\{1+0.0063 \ e - e - 0.0008$
$$(760-b)\} \quad . \quad . \quad (2)$$

These two results may be regarded as practically identical, and are supported by those obtained by Messrs. Butterfield, Haldane and Trotter,* with whose formula the second one given above is identical.

A number of different determinations by various observers have now been made of the constants of the pentane lamp for changes of humidity and atmospheric pressure, and these are scheduled in the following table :—

TABLE VII.

Observer.	Date of publica-tion.	Lamp.	Value of constants.	
			Humidity.	Barometric pressure.
a Liebenthal	1895	1 c. pentane	0.0057 +	0.0005
b Paterson	1904	10 c. ,,	0.0066	0.0008
c Dow	1906	,, ,,	0.0071	0.0008
d Rosa and Crittenden	1910	,, ,,	0.0056	0.0006
e Butterfield, Haldane and Trotter	1911	,, ,,	0.0062	0.0008
Paterson and Dudding	1914	,, ,,	0.0063	0.0008

a Liebenthal, " Zeitsch. für Instrumentkunde," 1905, p. 157.
b C.C. Paterson, " Proc." I.E.E., 1907, p. 271.
c J. S. Dow, " Elec. Review," Sept. 28, 1906.
d Rosa & Crittenden, " Trans." Ill. Eng. Soc., N.Y., 1910, p. 753.
e Butterfield, Haldane and Trotter, " Journ. Gas Lighting, 115, p. 290, 1911.

The tentative figure of 0.006 given by Rosa and Crittenden for the barometric correction must not, as stated by them in their Paper, be regarded as accurate, and the figures obtained by Liebenthal were for a 1 candle lamp of entirely different construction. Hence, neglecting these, it will be seen that

* Butterfield, Haldane and Trotter, " Journ. Gas Lighting," 115, p. 290, 1911.
† Based on a normal humidity of 8 litres per cubic metre.

there is general agreement as to the correcting factor to be applied for barometric changes. There is not, however, the same close agreement between the determinations of the humidity factor. Although the actual candle-power differences represented by the discrepancies between these different constants are small over the ranges of humidity met with in this country, the accuracy of most of the determinations would have justified the expectation of closer agreement in the value of this factor.

The work of Rosa and Crittenden at the Bureau of Standards, Washington, is of the highest accuracy, and the difference between their value and those determined in this country by the authors and by Messrs. Butterfield, Haldane and Trotter cannot in either case be ascribed to experimental error. In the publication of their work* Rosa and Crittenden make two suggestions as to how the difference might be accounted for, but they had not then before them the confirmatory evidence of the more recent investigations in progress in this country at the time they wrote.

It would seem that a definite difference in the conditions in Washington and London must be looked for to account for the discrepancy, and the present authors think it will be found to lie in the fact that the pentane lamp really has a temperature coefficient, but that its effect is almost entirely masked by the method generally used of determining and applying the humidity correction. In a more recent publication of the Bureau of Standards,† Messrs. Crittenden and Taylor suggest the possibility of a temperature coefficient, but they do not discuss its bearing on the constant for the humidity correction. In their earlier publication also they mention the effect which the temperature may have on the humidity correction, as determined in the earlier N.P.L. investigation.‡

The authors have not been able to determine the temperature coefficient of the pentane lamp. The difficulties found by all investigators, and mentioned by Crittenden and Taylor, of changing humidity and temperature independently have pre-

* *Loc. cit.*
† " The Pentane Lamp as a Working Standard," Crittenden and Taylor, " Bulletin " of the B. of S., Vol. X., p. 410.
‡ In a still more recent publication (" Bull. B. S.," Vol. X., p. 574), received after this Paper was written, the same authors refer to the subject again, and make the suggestion which has been elaborated here, but without giving actual figures connecting humidity and temperature at Washington. If these are obtainable and could be compared with those given here, the validity or otherwise of the suggested explanation would be established.

vented this. Nor is the matter of sufficient importance to justify the prolonged and difficult investigation which would be necessary. On the assumption, however, that there is a temperature coefficient the work already done enables an estimate to be made of its influence.

If it may be assumed that the candle-power of the pentane lamp under various atmospheric conditions is given by

$$\text{Candle-power} = 10 + A(8-e) + B(760-b) + C(*12-t), \quad . \quad . \quad (3)$$

where A, B and C are constants.

e = litres of water vapour per cubic metre,
b = height of barometer in millimetres.
t = temperature in °C.

The method of least squares enables us to determine the most probable value of the constants A, B and C from a large number of simultaneous observations of the variables, candle-power, humidity, barometer and temperature provided that : —

(a) All values of humidity, barometer and temperature are equally likely to occur conjointly : or (b) if a definite relation exists between any of the variables not satisfying condition (a).

An examination of the observed values of humidity, temperature and barometer, reveal that condition (a) is fulfilled by the values of humidity and barometer or by the values of temperature and barometer, but that the humidity and temperature values satisfy neither condition (a) nor condition (b). This last fact is illustrated in Fig. 4, where the simultaneously observed values of humidity and temperature obtained whilst observing the candle-power of the pentane lamp both in 1904-06 and 1912-14 are plotted. It is seen that a loose general relation exists between these variables, the lines A and B representing the best linear relation for the two sets of experimental data, and so to some extent condition (b) is fulfilled.

If, now, it is assumed that the linear relationship shown by lines A and B in Fig. 4 actually exists between these values of humidity and temperature, the following are the values which result for the coefficients A, B and C in the above equation (3) :

$$\text{C.P.} = 10 + 0.187(8-e) - 0.008_5(760-b) - 0.076_5(12-t). \quad . \quad (4)$$

Two facts should now be observed. Firstly, the temperature and humidity effects act against one another, and in practice it is the difference between the two which is operative. Secondly, as at Teddington, an increase of one unit of water

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* The normal temperature of 12 deg. is chosen because it corresponds with the previously fixed normal humidity of 8 litres per cubic metre.

vapour is accompanied on the average by a rise of $1\cdot6_2°$C., the combined humidity-temperature coefficient becomes

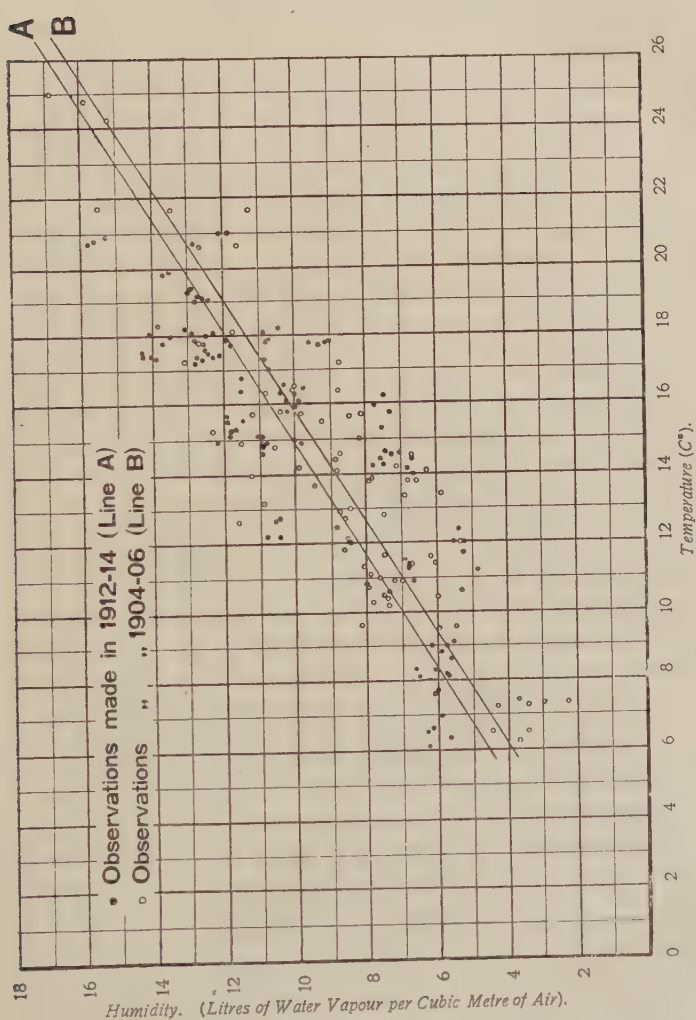$$0\cdot187-(0\cdot0076_5 \times 1\cdot6_2)=0\cdot063,$$

FIG. 4.—DIAGRAM SHOWING VALUES OF THE TEMPERATURE AND HUMIDITY OF THE AIR OVER THE PERIOD DURING WHICH OBSERVATIONS WERE MADE ON THE CANDLE-POWER OF THE PENTANE LAMP.

Observations made in 1912-14 (Line A)
Observations ,, 1904-06 (Line B)

Temperature (C°).

Humidity. (Litres of Water Vapour per Cubic Metre of Air).

viz., the coefficient in equation (2). Now the coefficient given by Rosa and Crittenden is $0\cdot056_7$, and it is readily seen that this would result from a prevailing climatic condition in which

an increase of one litre of water vapour per cub. metre corresponds with an increase of 1·7 C., instead of the 1·6₂C. observed at Teddington. It is obvious that too great a significance must not be attached to the actual values of the coefficients in equation (4), since they depend on the assumption that a linear relation connects humidity and temperature.

The difference between the " humidity " coefficients determined (by neglecting the temperature coefficient) in England and America being much larger than that which could be attributed to the error of the experiments, tends to support the suggestion of the authors that the pentane lamp has a temperature coefficient, but that the usual method of making the observations and deducing the results does not allow of its determination. Thus, it would appear that the " humidity " coefficients determined for flame standards are really combined humidity-temperature coefficients. Whenever, therefore, a lamp is used under conditions of humidity and temperature which approximate to those existing at the locality where the original determination was made, the constant so determined will apply rigidly. If, however, the determination of the combined humidity-temperature coefficient be made under different climatic conditions, a slightly different constant may be expected. If, for instance, the humidity at Washington tends on the whole to increase at a different rate with temperature than it does at Teddington a different factor for the combined effects would be expected to result. The authors are not in a position to know if this is actually the case, but it is not unreasonable to suppose that differences of the order indicated might be found to exist, and if this should be so it would afford an explanation of the difference which has been found between the " humidity " coefficients determined .in the two localities.

The conclusion is, that if work of the very highest accuracy is to be carried out with flame standards under abnormal humidity conditions the combined humidity-temperature coefficient should be determined for the locality in which the work is to be conducted. It should be pointed out that the difference between the American and English determinations for the pentane lamp amounts to less than 1 per cent. in candle-power for a rise of humidity of 10 litres per cubic metre above the normal. Values have seldom been observed greater than this in Teddington. The table at the conclusion of the Paper

by Crittenden and Taylor shows the average humidities in
Boston and New Orleans to be 9·9 and 19·1 litres per cubic
metre respectively. Where such large differences exist in
climatic conditions the question of the variation of the com-
bined temperature-humidity coefficient might with advantage
be further investigated.

(6) *The Constancy of the Unit of Candle-power held in Electric
Sub-standards.*

The continual use which has been made of electric sub-
standards since the first determination of the unit of candle-
power in 1904, has afforded opportunities of watching the
behaviour of such lamps and particularly of observing their
constancy.

Several observers have written on this subject, amongst
whom are J. A. Fleming, C. H. Sharp, P. S. Millar, E. B. Rosa,
G. W. Middlekauff,* and others, and all witness to the con-
stancy of properly prepared and seasoned electric sub-standards.
Their observations have been mainly concerned with carbon
filament glow lamps, and the present authors are able to en-
dorse the views they express. In order to keep records of
their behaviour, an annual analysis is made of all the photo-
metric records where sub-standards have been used during
the year. When an ordinary routine standardisation has to be
made, three to six sub-standards are put on the bench in turn,
and thus for each day's work a value is obtained for any one
standard lamp in terms of others of the same set. The analysis
of such results over a year's working indicates if any individual
lamp shows signs of differing from the mean of the others. It
has been unusual to find that a lamp has appeared to change
during 12 months by more than 0·1 per cent., and the majority
of them show no change which can be detected. If any lamp
in a set shows a difference from the mean of 0·1 per cent. no
change in the value assigned to it is made until such difference
is repeated in the following year's analysis. Of the one or two
lamps whose values have had to be adjusted in this way, some
appear to have risen and some to have fallen in candle-power,
and there is no sign that any fundamental set of lamps is
undergoing a progressive change. This can be said equally of
the tungsten filament lamps as of those with carbon filaments.

* Fleming "Proc." Brit. Assn., 1904. Sharp and Millar, "Trans." Ill.
Eng. Soc., N.Y., June, 1910. Sharp "American Gas Inst.," Oct., 1913.
Rosa and Middlekauff, "Proc." Amer. Inst. Elect. Eng., July, 1910, p. 1911.

A set of 1·5 watts per candle tungsten standards (set 6) has to be used nearly every day of the week, and a fundamental as well as a working set of these lamps is, therefore, kept. The working set fell in candle-power about 0·5 per cent. during the continuous use of the past two years. As the fundamental set will only be used perhaps two or three times a year, its constancy for very many years is assured. It could at any time be compared against set 5 (2 watts per candle), which has never to be used in ordinary routine work, and which, as a matter of fact, has not been used since the values of its individual lamps were first fixed by the cascade method. Similarly, the use of set 1 (matching the pentane lamp in colour) will never be necessary in ordinary work because of the redness of the light. When it is remembered that the filaments of these lamps are of tungsten operating at the low efficiency of about 7 watts per candle, and that they only require to be used for exceptional reference purposes at intervals of several years, there would seem to be every reason for expecting the unit of candle-power to be maintained constant by means of them for an indefinite period.

The authors desire to place on record their obligations to Dr. R. T. Glazebrook, C.B., F.R.S., Director of the National Physical Laboratory, and on behalf of the Laboratory to acknowledge the generosity and help of the General Electric Co. and the Osram Lamp Works for the many expensive and special standard lamps which they have made and presented in connection with this work.

DISCUSSION.

Mr. A. P. TROTTER said that an incidental result of the work described in this Paper was to reveal the precision with which photometric measurements are now made, and to raise the status of that operation. He wished to confine his observations to the moisture coefficient for the pentane standard, and to the possibility of the existence of a temperature coefficient. *A priori* considerations seemed to suggest that an appreciable temperature coefficient would exist. Pentane is a highly volatile substance. In the absence of a wick, the feed depends on the volatilisation of the pentane, and on the siphoning of the heavy vapour. Small departures from the standard pattern materially altered the flow of air and vapour by introducing thermal differences. But though a temperature coefficient has been looked for it has not been found experimentally. The authors suggest that the lamp really has a temperature coefficient, but that " its effect is almost entirely masked by the method generally used for determining and applying the humidity correction," and that " temperature and humidity effects act against one another." They say that if " the humidity at Washington tends on the whole to increase at a different rate with temperature than it does at Teddington, a different factor for the combined effects would be expected to result." This looks at first as though it were suggested that European laws of

hygrometry and dew-point did not hold good in America. The explanation probably is to be found in the degree of saturation. At low temperatures the moisture in the air is low in both climates, but at high temperatures the air is less saturated in Washington than at Teddington. In that case the effects of temperature would be more marked at the former place than at the latter. Both the variations of atmospheric pressure and of moisture in the case of the American observations and of those at the National Physical Laboratory were the result of natural metereological changes. The range of humidity at Washington varied from 0·4 per cent. to 2·7 per cent. by volume. The variations in the case of the experiments of Butterfield, Haldane and Trotter were for the most part produced artificially, and the ranges were much larger. Atmospheric pressure, humidity and vitiation by CO_2 were varied separately, one effect at a time, the other two remaining normal. Atmospheric pressure was varied from 450 mm. to 1,000 mm. Moisture varied, in the case of the pentane lamp from 0·73 per cent. at a temperature of 6·6°C. to 4 per cent. at a temperature of 33·2°C. It was not considered safe to use the pentane lamp at a higher temperature, but other experiments were carried on at 44°C. The moisture was produced by a flow of steam from a boiler outside the room, and the temperature was raised by electric heaters and gas burners. A saturation of about 90 per cent. was reached. After applying the corrections for atmospheric pressure and for CO_2, both of which had been well ascertained, it was found that the difference of light due apparently to moisture could be accounted for by a correction employing the coefficient 0·00625. The mean residual error was about 1·3 per cent. Experiments were made at temperatures varying from 8°C. to 20°C., the other conditions remaining approximately normal. the humidity varied only from 0·84 to 1·21 per cent., but without producing any material alteration in the light. Since the humidity at nearly all temperatures may be assumed to have been greater in the British than in the American investigations, it follows that the temperature will have had less relative effect on the British than on the American humidity coefficient. Hence, if the authors' assumption that there should be a temperature correction is valid, it is obvious that the British humidity coefficient is more nearly correct for humidity *per se* than the American. The majority of the observations from which the humidity co-efficient has been deduced here, and probably in America also, were made at higher temperatures than 15°C. (which presumably would be the accepted standard temperature), consequently if the temperature effect is in the opposite sense to the humidity effect the true humidity coefficient would be something higher than the British figure of 0·00625. . The difference between this figure and the American coefficient 0·00567, viz., 0·00058, may be taken as a first approximation for the temperature coefficient. It cannot, however, be expressed in terms of temperature without more information about the temperature at which the American observations were taken. Experiments are needed to settle this. They should be conducted in winter in a chamber provided with the necessary arrangements for withdrawing the products of combustion, and with means for raising the temperature while maintaining as low a degree of humidity as possible.

Mr. J. S. Dow congratulated the authors on the painstaking work described in the Paper. The fundamental difficulties involved in comparing heterochromatic sources of light have not yet been overcome, nor the complex problems settled ; but such devices as the use of coloured screens seemed to assist those who had not much experience of such work, and no doubt a series of carefully prepared standards, such as those mentioned by the authors, would be of service. Nevertheless, it is doubtful if such methods are worth the trouble involved in the case of fairly expert operators. The various observers using the cascade method did not seem to agree much better than those relying on direct comparison. If due regard is paid to the elimination of personal errors by taking sufficient observations, direct com-

parison is the simplest and perhaps the most reliable method. The cascade
and other methods are of scientific interest as a "check." In view of the
minute differences with which the authors worked he would like to ask if an
appreciable difference in heterochromatic work could not be obtained by push-
ing in and out the telescope of the Lummer-Brodhun photometer. Some
experiments he made about eight or 10 years ago had led him to think that an
appreciable difference could be obtained in this way. Also, in comparing,
say, a tungsten lamp with the pentane, a distinct difference might be noted
in the results obtained with three good photometers of different types, taken
at random ; for the size of the photometric field affected the physiological
impression of brightness. The authors had referred to the effect of humidity
on the pentane standard. Some very good work on this subject had been
done in the States where the wide climatic variations were favourable to
such researches. These results seemed to confirm Mr. Paterson's investi-
gations ; the chief difficulty in America at that time seemed to be to get
exactly reproducible lamps. He would like to know how close the lamps in
this country now agreed. Some very careful comparisons between the
flicker and the equality of brightness photometers had recently been made in
the United States, and the tendency is to credit the flicker photometer with
greater reliability in colour work. Personally he would prefer an instru-
ment of the ordinary type—except possibly when very violent colour—
contrasts were in question.

Mr. A. CAMPBELL also thought that the figures in the Paper did not justify
the conclusion that the cascade method was much superior to the direct
comparison of the sources.

Mr. T. SMITH (communicated remarks) asks the authors if, apart from the
reduction in the differences between different observers, any advantage is
obtained by the adoption of the cascade method regarding it solely as a means
of finding the candle-power of lamp 6 in terms of that of lamp 1. It would
be of interest to see a curve plotted showing the relation between the candle-
power found for lamp 6 from that of lamp 1, and the number of intermediate
lamps used in making the comparison. The figures given in the Paper would
determine the extremities of such a curve, but further information was
required before more than a conventional meaning could be assigned to the
figures obtained for the candle-power of the lamps higher in the series. The
curve suggested should help to show whether direct comparison or cascade
measurements were to be preferred in the determination of standards. A
comparatively small number of observations would determine the form of
this curve. The photometric comparison of lights of different colours may
be affected by the chromatic aberration of the eye. The difficulty of de-
ciding when two colours are equally bright may be partly due to the eye
requiring to accommodate itself so as to bring each colour in turn into sharp
focus on the retina. It would be of interest to compare the results described
in the Paper with others made with the same lamps and by the same method,
but with a lens placed in the photometer head just in front of the eye and so
constructed as to cause the photometer contrast pattern to be in good focus
on the retina simultaneously for practically the whole extent of the visible
spectrum ; no magnification would be produced. The lens would be correct
for all observers, and the results obtained would be of interest, apart from the
possibility of reducing the uncertainty in measurements of candle-power.
Perhaps some Fellow of the Society could supply quantitative details of the
chromatic aberrations of the eye from which such a lens could be computed.
Such details did not appear in the standard works on Physiological Optics.

Mr. J. GUILD (communicated) : As pointed out by the authors, it appears
to be necessary even in comparing ordinary "white" lights to stipulate
normal colour vision on the part of the observer, and the cascade method of
subdividing colour differences, while minimising the probable errors of
observation, leads apparently to the same result with a particular observer
as should be obtained by direct comparison. The authors quote results from

six observers, but to what extent one is justified in assuming the average colour vision of that group to agree accurately with that of another similar group which might be chosen at some other standardising institution it is difficult to say, and it seems highly desirable to have some method by which light may be measured absolutely rather than by comparison with arbitrary standards. In the " Proceedings " of the Royal Society (Vol. LXXXVII., 1911, p. 275) Dr. Houston proposes to reproduce the selective sensitivity of the eye to energy of different wave-lengths by a suitable light filter of which the transmission curve is similar to the sensitivity curve of the normal eye and to measure the transmitted energy thermo-electrically. The arrangement virtually constitutes a standard "eye" which has the property, not possessed by natural eyes, of measuring the intensity of the light which it receives. Have the authors considered the proposal ? The authors state the approximate illumination at the photometer head throughout their work. This is a useful practice. As is well known, the sensitivity curve of the eye for the visible spectrum varies with the intensity of the incident light, the red end being penalised as the general brightness is reduced. Hence, in comparing two sources of different hue, a lower value will be ascribed to the redder light the further we remove the sources from the photometer. The cascade method of subdividing the colour gap will not get over this, and it would be of interest to know what range of illumination at the photometer might be employed in the subsequent use of the authors' sub-standards without introducing errors comparable with the degree of accuracy aimed at. Pulling out the draw tube of the photometer, by reducing the brightness of the retinal image, would produce the same result as moving the sources further off, and this might account for the effect observed by Mr. Dow, since the angular size of the field of the photometer seems to preclude the explanation that this is due to the varying colour sensitivity of different parts of the retina. Errors of this description may be wholly insignificant within the range of feasible experimental conditions, but the advent of such high precision in photometry makes an investigation of the possible magnitude of such errors desirable. Until such an investigation is made it appears to be a necessary precaution, when using any sub-standard source of light, to use it at a constant distance from the photometer and with the same type and size of photometer as when the lamp was standardised.

Mr. C. C. PATERSON, in replying for the authors, noted that Mr. Trotter agreed that it was possible that the relative degree of saturation in America and London would account for different correcting coefficients, if it was found that the pentane lamp actually had a temperature coefficient. Mr. Trotter seemed to imply that from his experimental results covering a larger range of humidity and temperature it might be possible to deduce the temperature coefficient. In order that this could be done one wanted as many observations where there were low humidities and high temperatures as there were observations in which both humidity and temperature were high. This is a condition which up to now it has been found impossible to obtain, and it would seem necessary to fall back on the method suggested in the Paper in which the conditioned relationship between humidity and temperature was determined, and by using this in the method of least squares to obtain an approximation to the temperature coefficient. The authors do not think it possible to determine the magnitude of the temperature coefficient in the way suggested at the end of Mr. Trotter's remarks. As the combined humidity-temperature coefficient is probably the result of the algebraic sum of a positive temperature coefficient and a negative humidity coefficient, the actual temperature and humidity-coefficients would be expected to be of quite a different order from the combined one. As shown in the Paper, the humidity-coefficient of $+0.187$, combined with a temperature coefficient of -0.076, would fit the experimental results quite well instead of the present combined coefficient of $+0.062$. In reply to Mr. Dow and also to Mr. Campbell, the somewhat laborious cascade method of com-

parison was only suggested as a means for a central laboratory to establish and standardise its sub-standards of light of different hues, so that standard lamps could be issued to other testing-rooms the colour of whose light was suitable for the work done. It was quite true that the sets of high efficiency sub-standards now used at the National Physical Laboratory for such standardisations would have been given substantially the same candle-power values when standardised directly against the primary standards as they have actually been given by the cascade method. But this could not be foreseen. Nor would the former method have given sets of standards of intermediate colours, against which the high efficiency standards can be checked back at any time, or against which other lamps of intermediate efficiencies could be standardised. A set of high efficiency sub-standards whose values are known in terms of the average eye is essential, otherwise all ordinary standardisations and photometric measurements would have to be carried out with a large colour difference, which would involve serious errors unless all measurements were made by a large number of observers. Perhaps Mr. Dow had not kept sight of this in his observations. The question of the size of the photometric field raised by Mr. Dow is an interesting one. The authors had also made observations of this effect and found differences where the colour contrasts were very great and the distance of the eye from the photometer varied over wide limits. As Mr. Guild has pointed out, however, in an ordinary photometer the field of vision is relatively large. The result of this when working with the Lummer-Brodhun head is that an observer would not make a judgment of the field as a whole but would look at any one moment at one particular portion of it, and they would not have expected any different result if the field had been twice the size. As a matter of fact, in all measurements each observer adjusted the telescope so as to bring the field of view into proper focus, and differences due to variations in the size of field were not noticed. Where the colour differences are small, as in the cascade method, different photometers intelligently used on the substitution principle would probably agree. The authors are not able to say anything definite as to the reproducibility of the pentane lamp. All their work has been done with the standard at the National Physical Laboratory. Perhaps a rough figure of 1 per cent. might be taken for the closeness of reproducibility of the pentane lamp as at present constructed. In reply to Mr. T. Smith, it would be interesting to have the results he suggested ; but as any one set of photometric standard glow lamps cannot be expected to keep constant within 0·1 per cent. if too large a number of observations are made with it, it is impracticable to extend the number of observations indefinitely. A very large number of observations would be necessary for the accuracy to be comparable with that given in the Paper. The " artificial eye," mentioned by Mr. Guild, is a very attractive subject. The authors have not aimed at such a device. Sensitiveness is one of the difficulties. Such an " eye " must be adjusted and calibrated in terms of a large number of observers before its readings can be taken as having a photometric value, and it is therefore not more fundamental than the procedure suggested by the authors. The phenomenon discussed in Mr. Guild's concluding remarks appears to be a Purkinje effect. Mr. Dow has shown that this begins to come in at illuminations of the order of 0·1 ft. candle. It is very seldom that illuminations at photometers approach such low values. Photometric work tends to be carried out at increasing rather than decreasing illuminations, about 1 to 2 ft. candles being usual.

XXI. *The Relative Losses in Dielectrics in Equivalent Electric Fields, Steady and Alternating.* By G. L. ADDENBROOKE, *M.I.E.E.*

IN " The Electrician " for March 1, 1912, I described, I believe for the first time, the general relations between the losses in dielectrics in fields of equivalent voltage, continuous and alternating (R.M.S.), the latter having a range of periodicities of from 1 in 4 seconds to 42 $\sim$ per second, the dielectrics being in all cases in intimate contact with the electrodes.

As $R = \dfrac{E^2}{W}$, the continuous-current resistance or the corresponding specific loss can be compared with the alternating actual or specific loss, under similar conditions as regards voltage, it being understood that with continuous P.D.'s the field is maintained long enough for the deflection to become steady or nearly so.

In a communication in May, 1912, to the Physical Society, it was shown that similar relations exist in the losses occurring across the films of moisture which are present on the surfaces of dielectrics.

In both these communications attention was drawn to the variations in the losses caused by slight differences in the amount of moisture present, both in the magnitude of the losses and in the forms of the diagrams obtained by plotting them.

Lastly, in " The Electrician " for January, 1913, I described further experiments showing in detail the actions occurring in a fairly high-class dielectric—viz., gutta-percha at very low periodicities up to 42 per second.

Part of the data given in this latter communication have since been used by Dr. Ashton in his Paper last session on dielectric theory. Working on lines developed by Maxwell, Hopkinson, Pellat and Schwidler, and extended by himself, Ashton appears to be able to reproduce my curves fairly closely for periodicities above about two per second. Below this frequency, however, the discrepancies increase. I have re-examined these results since, and have also submitted certain others to Dr. Ashton, and it does not appear that there can be any error in the observations which is at all likely to account for the differences.

Again, for frequencies *above 6 or 8 per second* the curves of

losses at different frequencies are very well represented by assuming a fixed loss added to a loss which varies directly with the frequency, or is constant per cycle whatever the frequency. This very simple formula seems to represent the losses from the above to very high frequencies with considerable accuracy, but below these frequencies it fails, and the fixed loss which must be assumed on this basis is always larger, and usually much larger, than that given by the equivalent steady current ; in fact, the two seem with good dielectrics not to bear any relation to each other.

Nevertheless, in the course of measuring the alternating and continuous losses in a large number of dielectrics I almost subconsciously came to feel about two years ago that there was a direct connection, and I determined to look into the matter more closely.

It was obviously desirable to include results for a number of different dielectrics of widely different composition, and also to get the data on some simple and common basis, which would leave the ratios of the actions in the different dielectrics to stand out as clearly as possible. Finally, it seemed best to take the specific loss corresponding to a specific resistance of 1×10^{15} ohms per centimetre cube as a basis, and express all the losses as multiples of this. It seems better at this stage also to deal with both the continuous and alternating losses as dissipation of energy without postulating anything further as to whether it is of a conductive character or not.

As a commencement the continuous pressure loss is compared with the alternating loss at $40 \sim$ only ; the question of what the results would be had another periodicity been chosen will be considered later.

All the measurements, unless otherwise stated, were made by enclosing each dielectric in the form of a sheet, in a container, between mercury faces, at an average pressure of not less than 4 in. of mercury. The observations, where not otherwise specified, were all made at room temperatures, the two sets of observations on each substance one immediately after the other. The voltages used were between 100 and 120 to avoid any chance of heating or straining the dielectric, a point about which it is necessary to be most careful, particularly when the dielectrics are not of the highest class. The observations were made as described in the above communications with the latest form of my electrostatic wattmeter and a special form of reflecting electrostatic voltmeter.

In the tables the different dielectrics are arranged in the order of the specific resistances, at present their best known attribute, the values of which are given in the second column.

The third column gives the corresponding specific loss which corresponds with the conductivity.

The fourth column gives the corresponding specific alternating loss at 40 ∿ in the same units.

The fifth column gives the ratio of the specific continuous losses to the alternating for each separate substance.

TABLE I.—*Connection between Continuous and Alternating Losses* (40 ∿), *specific, in Dielectrics, at the Same Equivalent Voltages* (C. *and* R.M.S., 100-120 *Volts*).

Solids. Substance and state.	C.C. resistance in ohms per cm.³.	C.C. conductivity 10^{-15} mohs as unity.	A.C. conductivity 10^{-15} mohs as unity.	Ratio of losses for each substance.
Blotting paper, room dryness.......	1.3×10^{9}	770,000	1,150,000	1 : 1.5
Celluloid, room dryness, 0.62 mm.	1.7×10^{10}	59,000	118,000	1 : 2.0
Blotting paper, dried in sun	3.2×10^{10}	31,000	71,000	1 : 2.3
(a) Celluloid room, dry 0.4 mm....	3.2×10^{10}	31,000	65,000	1 : 2.1
(b) ,, ,, 0.23 mm....	5.7×10^{10}	17,500	44,000	1 : 2.5
Rubber-covered wire	1.2×10^{11}	8,000	64,000	1 : 8.0
(a) Celluloid dried, 0.4 mm..........	2.9×10^{12}	340	6,400	1 : 19.0
(b) ,, ,, 0.23 mm........	4.4×10^{12}	230	4,800	1 : 21
Rubber sheet water soaked, 12°C.	2.0×10^{13}	50	3,500	1 : 70
Crown glass, 11°C.	3.7×10^{13}	28	6,200	1 : 220
Rubber sheet, room dry, 12°C.......	5.0×10^{13}	20	1,800	1 : 90
Gutta-percha, 25°C.	2.0×10^{14}	5	1,750	1 : 350
Rubber sheets dried, 100°C. & 12°C.	2.6×10^{14}	4	1,600	1 : 400
Bohemian glass flask, 12°C..........	8.0×10^{14}	1.2	5,100	1 : 4,200
Gutta-percha, 11°C.	1.0×10^{15}	1.0	1,200	1 : 1,200
,, ,, 8°C.	1.7×10^{15}	0.6	1,080	1 : 1,830
Hetero	geneous Sol	id.		
Micanite	2.25×10^{14}	4.4	7,500	1 : 1,700

TABLE II.—*Liquid Dielectrics.*

Substance.	C.C. specific resistivity ohms.	C.C. conductivity 10^{-15} mohs as unity.	A.C. conductivity 10^{-15} mohs as unity 40 ∿.	Ratio of losses C.C. to A.C. for each substance.
Petroleum, ordinary	6.0×10^{10}	16,600	33,000	1 : 2.0
Mineral oil and petroleum mixed	2.4×10^{11}	4,100	15,000	1 : 3.7
Olive oil, as obtained..............	1.0×10^{12}	1,000	3,200	1 : 3.2
Mineral oil, thin	1.5×10^{12}	650	3,250	1 : 5.0
Castor oil, medicinal	2.0×10^{12}	500	2,500	1 : 5.0
Mineral oil, thick (1)..............	2.5×10^{12}	400	2,650	1 : 6.6
Above oil heated to 185°C. and cooled (2)	6.5×10^{13}	15	1,950	1 : 130.0

The chief points brought out by these tables are :—

(a) That the order of the alternating losses follows so closely the steady pressure losses that there can be little doubt that the two are different aspects of phenomena arising from the same cause, and this applies both to liquid as well as solid dielectrics.

Out of the 24 examples given in the tables there are only three exceptions—micanite, crown glass and thin blown bohemian glass. Micanite was specially chosen as a clearly heterogeneous substance. The mica in this has a very high specific resistance and very small alternating losses, but the shellac has a little moisture in it, and should show correspondingly high losses in an alternating field. If then the losses are due to the continuous movement of ions, or, possibly, of free electrons through the dielectric in a steady field, we should expect, in this case, that having for the most part to make their way deviously round the edges of the mica flakes, their motion would meet with an inordinate amount of resistance in comparison with the thickness of the dielectric. And this is just what we see happens. On the other hand, with an alternating field the motion being reciprocal and through relatively very short distances would be little affected by the presence of the mica, and would be relatively large. And this again is what we see takes place.

As illustrating this and some other effects, the following curves are interesting. The upper curve shows the relative losses, steady and alternating, in a sheet of celluloid of room dryness over a range of frequencies. The lower curve shows the effects when the same sheet of celluloid, which was 1·4 mm. mils thick, was sandwiched between two sheets of gutta-percha each 0·06 mm. thick. In the latter case it will be seen that loss due to the steady field is reduced practically to nothing, between this and 16 periods there is considerable reduction in the losses, but between 16 and 40 periods in each case the curves are practically straight lines and parallel to each other. The portion of the alternating losses, which is indicated by b, a constant and n the frequency or bn, is, therefore, sensibly the same in each case, and is unaffected by the presence of the thin sheets of gutta-percha. To get a correct result it is, of course, necessary to make a proper allowance for the presence of the gutta percha sheets which, increasing the thickness and introducing another substance of different dielectric constant, reduces the inductive effect by a small percentage. Below is

added a third curve to show the effect of drying.   All these
curves are to the same scale of ordinates.   Nearly identical
curves may be obtained by sandwiching a sheet of mica or
gutta percha between two thin sheets of celluloid or blotting
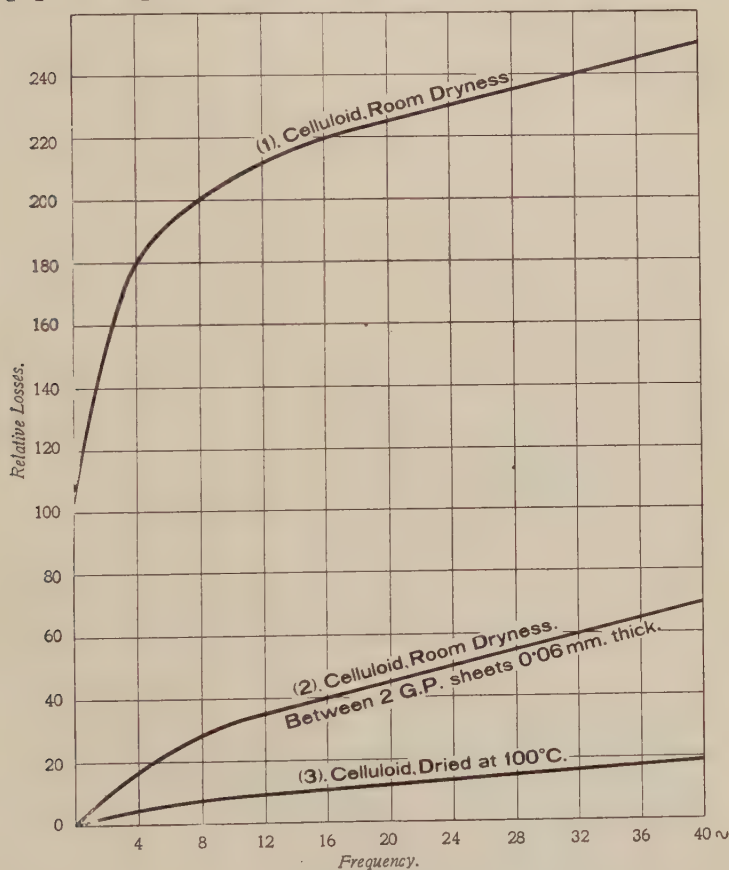paper, the point being that if there is a serious obstruction

DIAGRAM I.
Celluloid (1·4 mils thick).
Comparative losses, alone and between thin G.P. sheets.

anywhere or of any kind, the normal relations between the
losses in equivalent steady and alternating fields will be
altered, and while the steady field loss will be reduced the
alternating action will be less affected, so that the ratio of the
two to each other is increased.   I think, therefore, that we

shall not be wrong in attributing the differences observed with
the two glasses in the above tables to a similar cause, when we
consider that glass is naturally of a crystalline nature, and the
great strains and deformation which must occur when it is
blown or rolled in thin sheets. In these two cases the glass
flask was much thinner than the crown glass, and the strains
in it probably much greater, and we see that it deviates more
from the normal than the crown glass. I think, therefore,
that, except in cases the reasons for which can be seen, there
is fair evidence that the actions in steady and alternating fields
are very intimately connected and probably arise from the
same fundamental causes.

In the above tables I have not dealt with the very highest
class of dielectrics. It is so difficult without the most elaborate
precautions to get trustworthy results, but if the results of
Grover and Curtis, for mica and paraffin, giving the power
factors, are worked out and compared with the best determina-
tions of the resistances (for that of mica, *see* Campbell, Phys.
Soc. " Proc.," 1913) of these materials it will be found that they
lead to results of the same order, and seem to be in accord with
the general conclusions derived from the above tables.

It may be argued that there has been some selection in the
above specimens of dielectrics. On this I would say that
altogether I have tested at least twice the number of specimens
quoted in the list, and in some cases several different samples
of a particular substance, but they all seem to lead to similar
results, though varying in detail. Again, the above results
have not been hastily arrived at, since it is now three years
since I began to accumulate data on the lines given above, and
it is two years since in the description of the actions in gutta
percha mentioned above I drew attention to the fact that my
results seemed to point to such a connection as I have now
been able to demonstrate in detail.

There is, however, another criticism which may be made,
and which needs further careful examination, and particularly
as it gives an opportunity of bringing out some further and, as
it seems to me, interesting and important points. It may be
objected that while the results apply to the ratios of the losses,
with steady pressures and at 40 $\sim$ the sequence might be by no
means so obvious if some other periodicity were taken.

To deal with this point it is necessary to consider the losses
not only at one but at several different frequencies. For this
purpose I have selected five examples as typical. They in-

clude the first and last of the examples given above and three
intermediate ones, including crown glass. The full curves for
the losses in these are given below. As, however, owing to
the greater range of the losses these cannot be got into a dia-
gram to the same scale, and therefore a casual inspection of the
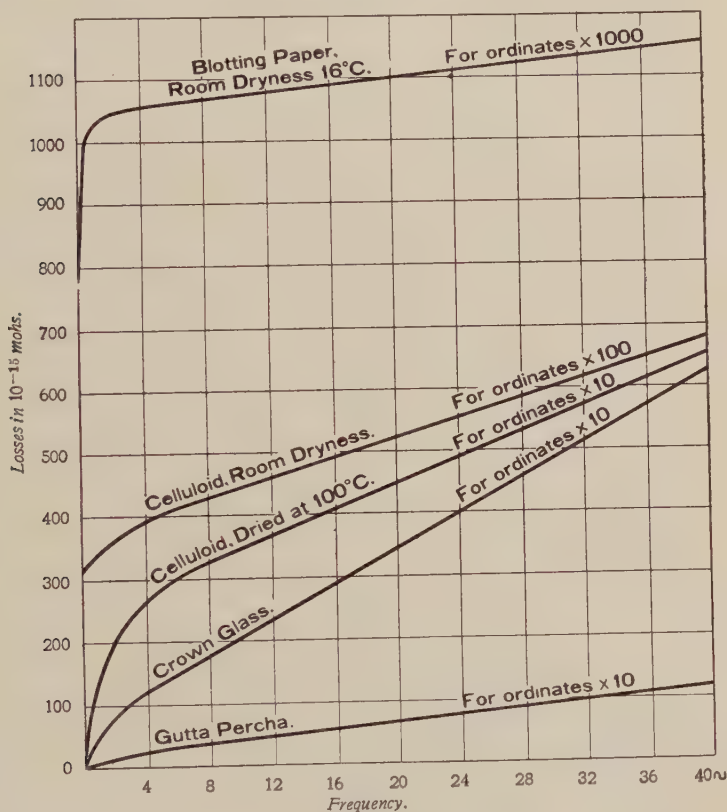curves does not convey a proper idea of the proportionality

DIAGRAM II.

Losses in dielectrics in equivalent steady and alternating (R.M.S.) fields.
Frequencies 1 in 4 secs. to 40 ∿.

of the actions. I subjoin below the data from which the
curves were drawn.

The first point is that above 8 to 12 periods per second the
rising curves are practically straight lines in all cases, and
whether the dielectrics are good or poor. In this diagram the

TABLE III.—*Specific Losses in Dielectrics, taking the Loss through 1×10^{-15} Mohs as Unity.*

Substance and state.	Sect. Mils.	Temp. °C.	Losses, steady field.	Losses, alternating fields and frequency.								
				~0·25	~0·5	~1	~2	~4	~8	~16	~25	~40
Blotting paper, room dry.	25	16	770,000	...	1,000,000	1,025,000	1,035,000	1,055,000	1,070,000	1,100,000	1,120,000	1,150,000
Celluloid, ditto............	16	15	31,000	32,000	33,000	34,000	35,700	39,500	42,500	49,500	55,500	65,000
,,　dried 100°C.	16	15	340	...	...	1,250	2,000	2,550	3,250	4,000	4,900	6,400
Crown glass	66	11	28	200	310	510	800	1,100	1,800	2,800	4,000	6,200
Gutta percha............	2·5	11	1	10	19	36	75	165	330	550	800	1,200
					Liqu ids.							
Thin mineral oil............	...	...		...	11,000	11,500	12,500	13,200	13,600	13,900	14,600	15,400
Castor oil	...	...	4,200	...	...	...	...	...	...	...	...	...
Mineral oil, dried............	...	...		...	...	...	...	...	...	...	...	...

curves end at 40 periods, but for the past 18 months I have had
an alternator capable of giving 100, 200 and 400 periods, and
have made numerous tests with this. In all cases the curves
continue sensibly as straight lines. Further, in one case, with
celluloid, through the courtesy of the Research Department of
the G.P.O., and with the help of their Mr. C. E. Hay, we have
been able to carry the tests up to 2,000 $\sim$. As far as can be
judged the curve is practically straight from, say, 12 $\sim$ per
second up to this periodicity. In some cases I have also been
able to fit results of my own on to the results of Fleming and
Dyke (*see* their Paper I.E.E. " Proceedings," 1912), and as far
as can be judged from such rough tests, since they do not give
the ordinary resistances, my curves can be directly prolonged
into theirs at frequencies of from 900 to 4,500. It seems clear,
therefore, that this is a general law for dielectrics of all classes—
viz., that above about 8 to 12 $\sim$ the curves of their losses at
varying periodicity are sensibly straight lines at any rate up to
telephonic periodicities. From above this low periodicity we
can, therefore, represent them with good approximation by
the simple formula $a+bn$, where a and b are constants.

To arrive at the constants from my data I take the losses at
16 periods and deduct this from the losses at 40 periods, the
remainder is then divided by 16, this gives us b; 16 times b is
then deducted from the loss at 15 periods. The remainder is a.

Below are given the figures for the curves in the diagram
calculated on the above basis and in the same units as before.
For comparison with them I have also added the continuous
or steady field losses on the same basis.

TABLE IV.—*Of Losses in Dielectrics.*
Values of constants a and b and steady field loss.

Substance and state.	Constant a.	Constant b.	Comparative steady field loss.
Blotting paper, room dry.	1,050,000	2,500	770,000
Celluloid „ „	36,625	735	31,000
„ dried 100°C.	2,450	100	340
Crown glass	700	137	28
Gutta percha	113	27·2	1
Liquid.			
Thin mineral oil	13,150	56	4,200

Derivation of constants :—
From loss at 40 $\sim$.
Deduct loss at 8 $\sim$.
Difference $\div (40-8)=32=b$.
Loss at 8 $\sim -8b=a$.
Loss at any frequency above 8 $=a+bn$ approximately.

It will be seen that while at the lowest resistance the a constant does not differ much from the steady field loss, the difference rapidly gets greater as the resistance of the dielectric is higher till the two seem to have no relation, although the data given in the table show unmistakably that they have. From these figures it will be seen that the curves do not cross each other at higher frequencies unless the dielectric is abnormal, and that the above relations generally still hold good.

Another point not clearly seen in the curves, but which can be readily deduced from the tables, is the forms the curves assume near their origin. Although from the general reduction of scale the curves seem to become flatter as the resistance becomes higher, this is not so comparatively. If in each case the steady field loss is taken as unity and curves of the comparative losses are drawn for the different periodicities the rise and sweep of the curves is greater comparatively as the resistance is higher. There are, however, differences in detail which would repay further study. Heterogeneity or great strain also affects the lower or b part of the curve. I have got out comparative data for the Bohemian glass flask from 1 to 200 periods. In this extreme case the a constant is almost negligible, and the very simple formula bn fits fairly and to a first approximation the whole curve from 1 to 200 periods. Even a single degree's temperature difference also considerably affects the exact shape of the low part of the curves. The effects of temperature are, however, too complicated to be dealt with in this Paper, especially as they do not seem to affect the general conclusions to which the above experiments lead.

When it is remembered that in the origin of these curves we are covering from another aspect the phenomena which have been studied under the term absorption, and when it is also considered that this term also covers the complementary capacity effects, there is no need to wonder longer why the elaborate attempts of numerous workers to find some formula of a fairly simple character to express their results have not met with success, and must be looked on, in the present state of our knowledge, as highly empirical.

Liquid Dielectrics.

In their general features the actions in liquid dielectrics in electric fields are the same as in solids, as can be seen from the data given above (Table II.). There are, however, charac-

teristic differences in detail. In the first place the " Blotting paper " type of curve persists more with liquid dielectrics, that is, at low frequencies the curves tend to be more square-shouldered. Then, for a given specific resistance the ratio of the steady to the alternating losses is generally less than in the case of solids ; this apparently is the most characteristic difference. I am sure, however, from my results that further detailed study would show other interesting deviations without upsetting the general resemblance in principle.

I have found, however, two difficulties in dealing with liquid dielectrics, especially the oils which make it less easy to get exact results than with solids. They very readily take up moisture from the atmosphere, which with dry oils alters their action while experiments are proceeding unless special precautions are taken against it. Secondly, there is another action when the oils have much moisture. I am then inclined to think that this moisture exists in the form of a fine emulsion in them. Such moisture existing then in globules is attracted to the electrodes wherever there is variation in the strength of the field. The result is that there is difficulty in getting a definite reading in steady fields ; it diminishes for some time after the field is put on, and this is not, I think, ordinary absorption. This action also occurs to a less extent in alternating fields of low frequency. It is necessary also to be very particular about the cleanliness and dryness of all receptacles used. It is, however, quite easy to get results sufficiently near to show the general nature of the actions such as those given. It is only when one comes to more precise measurements and the reproduction of results that the above actions interfere.

Range of Actions.

The alternating losses in different dielectrics have a much smaller range than those due to equivalent steady pressures, notwithstanding that the two appear to be intimately connected. The alternating losses are in all cases the larger, but whereas at 40 ∞ and over the wide range of dielectrics included in the above tables :—

The smallest A.C. loss has a ratio to the largest of   1 :   1,100
The smallest C.C. loss has a ratio to the largest of   1 : 1,300,000

Or the range of the alternating-current losses is only about 1/1,200th that of the continuous-current range.

Reference to the tables and data given above will show further that the range of the alternating losses is still further

reduced at higher frequencies ; in fact, to something like
1 : 300 over the above range before it becomes nearly con-
stant.

This difference in the two ranges of losses brings forward
the question as to what part of the ranges respectively it is
which show the greatest differences. It is at the lower resis-
tances or where the losses are greatest that the continuous-
current and alternating-current losses are more nearly pro-
portionate to each other, the difference becoming more
accentuated as the resistances get higher or, as it is called, the
dielectrics become more perfect. These interesting and sug-
gestive differences are best studied by careful comparison of
the figures given in the first two tables. When I first got out
these relations I thought it looked as if some intrusive sub-
stance were present which by drying or otherwise could be got
rid of, leaving a final action which must be on the substance
itself, or be a true dielectric hysteresis, but subsequent con-
sideration has led me to take another view.

To develop this a number of further experiments would
need to be described, covering the temperature, capacity and
pressure effects in steady and alternating fields and over a
range of periodicities, and also experiments on the effects of
moisture, in which I have endeavoured to get an idea of the
amount of moisture which corresponds with given magnitudes
of the actions occurring. The description of these experiments
must, however, be reserved for another Paper or Papers.

I have to thank Mr. C. W. S. Crawley for much kind help
and criticism in this work.

ABSTRACT.

After references to former work, especially on surface leakage,
tables are given showing an intimate connection between the losses
in steady and alternating fields, and that the one can be predicted
from the other to a first approximation. Exceptions are mentioned,
and it is shown that there is heterogeneity in these cases. Curves
of the losses from 1 in 4 seconds to 40∽ are given for specimen dielectrics.
Above 8 to 12∽ both for " good " and " poor " dielectrics, these become
rising straight lines. The salient features lie below about 16∽ .

The formula $a + bn$ applies to all dielectrics for frequencies above
about 8 ∽, but not below. The a constant is always larger than and
bears only a very indefinite relation to the steady voltage loss. Liquid
dielectrics behave similarly to solid with certain differences, particularly
that for a given resistance the ratio of the steady to the alternating loss
is less than with solids. There is a great difference in the comparative
ranges of the losses. For the dielectrics tabulated these vary in a steady
field from 1 to 1,300,000, but the corresponding alternating losses vary
only from 1 to 1,100. The difference is principally in the " good "

dielectrics, the alternating losses in which at 40 ∿ are vastly larger than these in a steady field, though they still appear to be connected with them.

DISCUSSION.

Mr. D. OWEN (communicated remarks) considered that the author's tests supplied data of great interest, though their value would be greater if more details of actual readings were supplied. The results support the $a+bn$ formula for dielectric power loss already advanced by previous workers. They show also that a and b are probably unconnected. This being the case, the Author has adopted a wrong basis of calculation in arriving at his main point. It would be better to show the relation between a and a_0 the direct current power loss. The discrepancies noted in regard to a few materials (good insulators) might thus be found to disappear.

Mr. E. H. RAYNER: The author mentions periodicities of one in four seconds. How are the voltage current and power measurements made under these conditions? Are the instruments so damped as to give a natural period of a minute or two? Even in such a case one would expect very inconvenient oscillations. Perhaps the author very greatly increases the inertia of the moving systems. Taking the continuous-current readings after the length of time required to become approximately steady is no doubt the best that can be done. It would afford much more information if intervals equal to a few periods of the corresponding alternating tests could be used. This would involve something of the nature of an oscillograph or an Einthoven galvanometer. As regards the curve 2 of diagram 1 I should have expected it to have been above curve 1 on account of the great resistance of gutta-percha to continuous currents. The remarks on the shape of curves obtained with liquid dielectrics are interesting. I can confirm the effect of electric stress on oil. When damp the globules of water seem to appear from nowhere and grow and wriggle under the electric forces. Finally they may cause a discharge and are boiled away, and so in time they may disappear in stages and the liquid will sustain a higher voltage than it would at first.

Mr. A. CAMPBELL (communicated): The careful observations recorded in the Paper are of value as widening our knowledge of the behaviour of dielectrics. It is interesting that in general there is no great change in the losses, as the frequency is lowered to 1 or 2 ∿ per second. A comparison of the steady current loss with the alternating appears to indicate that they have no clear connection. The practice, in the steady current test, of keeping the voltage applied until the apparent resistance has reached a steady value does not seem to give conditions comparable with those for the alternating voltage. Could the author, by means of his electrolytic commutator, go down to much lower frequencies, say, 1 ∿ in 100 seconds? By using a galvanometer of not too slow period and integrating the instantaneous product of the voltage and current curves the true alternating losses at such frequencies could be determined. The absorption effects in condensers certainly vary considerably over the first 100 seconds of charge. The existence of a state of strain in some of the glass tested, as suggested by the author, is rather improbable. This might be tested, however, with crossed nicols or the losses might be re-determined after re-annealing the glass.

Mr. G. L. ADDENBROOKE (communicated reply) expressed his obligation to those who had discussed his Paper for their suggestions. The difficulty was not to think of possible experiments but to select the most promising. He had not attached any physical meaning to the $a+bn$ formula. It was convenient to express the results over a certain range, but he thought a true solution would be found in some amplification of the formula employed by Ashton, and elaborated by other workers as far as possible with the data at their disposal. Such a final formula must include temperature coefficients, and will be of considerable complexity. In reply to Mr. Owen, the ratio of

a to a_0 is shown in the Paper for 40 ~ for five dielectrics, but a more precise study of this part of the curve would be useful. In reply to Mr. Rayner, there are four impulses in one direction per period in the case of the voltmeter and two for the wattmeter, so that the swing even at 1 ~ in 4 seconds is not so great as might be expected. The instruments are also made nearly dead-beat by a system of air damping. In using instruments of the galvanometer type, as suggested by Mr. Rayner and Mr. Campbell, there is a difficulty with some dielectrics in separating the losses from the capacity current even at very low frequencies. By the term " strain " a difference in structure or state of aggregation was intended rather than any mechanical strain, but he hoped to deal further with this point later.

XXII. *Second Report to the Council of the Committee on Nomenclature and Symbols.*

THE Committee consists of Prof. Sir J. J. Thomson, O.M., F.R.S. (President), Prof. H. L. Callendar, F.R.S., A. Campbell, Esq., B.A., Dr. C. Chree, F.R.S., Prof. G. Carey Foster, F.R.S., Dr. W. Eccles (Secretary and Convener), Sir George Greenhill, F.R.S., Dr. Alexander Russell, M.A., Prof. the Hon. R. J. Strutt, F.R.S., Prof. S. P Thompson, F.R.S., Dr. W. Watson, F.R.S.

In agreement with the recommendations of the International Electrotechnical Commission, the International Commission on the Unification of Physico-Chemical Symbols, and other bodies, the Committee recommend :—

That Italic, not Roman, letters be used as symbols for the magnitudes of quantities in all branches of Physics. This applies to capitals as well as to lower-case letters.

The following list of symbols for quantities occurring in the subject of Heat was adopted :—

Quantity.	*Recommendation.*
Reciprocal of mechanical equivalent of thermal unit $(1/J)$....................................	A
Constant in Van der Waal's formula $(V-b)(P-a/V^2)$ $=RT$	a
" Covolume " in ditto	b
" Coaggregation " volume$=a/RT$, in approximate formula $V=RT/P+b-c$	c
Cooling effect of Joule-Thomson$=(dT/dP)$ at constant total heat	C
Intrinsic energy	E
Thermodynamic potential, $T\varphi$-H, or H-$T\varphi$ if preferred	G
Acceleration of gravity	g
Total heat $E+PV$ of vapour	H
Total heat of liquid	h
Mechanical equivalent of thermal unit in gravitational units (*e.g.*, foot-pounds)	J
Thermal conductivity	K
Diffusivity	k
Latent heat of vaporisation	L
Mass or molecular weight	M

Quantity.	*Recommendation.*
Indices in such expressions as VT^m, PV^n	m, n
Pressure	P
Vapour pressure of liquid or saturation pressure	p
Quantity of heat energy	Q
" Dryness fraction " or " quality " of mixture of liquid and vapour	q
Gas constant (per unit mass)	R
Specific heat of vapour at constant pressure ...	S
Specific heat of vapour at constant volume, and specific heat of liquid or solid	s
Temperature reckoned from absolute zero......	T, θ
Temperature from freezing point or 0°C.	t, θ
Velocity	U, u
Specific volume of vapour or gas..............	V
„ „ liquid	v
Work	W, w
Entropy of vapour	Φ
„ liquid	φ
Ratio of specific heats	γ or n
Density	ϱ
Efficiency..................................	η

Nomenclature.

Recommended.—That the term calorie should not be used alone, but with a prefix indicating the unit of mass, the implied temperature scale being always Centigrade. *E.g.*, gram-calorie, kilogram-calorie, pound-calorie, which might be abbreviated to g.cal., kg.cal., lb.cal.

CONTENTS.